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FOREWORD

By Robert M. Yerkes

At Kindia, which is in French Guinea, approximately one hundred miles inland from the West African port of Conakry, in the year 1923 the Pasteur Institute of Paris established laboratories for utilization of monkeys and chimpanzees in the study of various problems of disease. This research organization, which is officially designated the Pasteur Institute of Kindia, has from its inception been developed under the direction of Doctor Robert Wilbert, veterinary officer of the French Army. For some years Doctor Wilbert has been ably assisted by Doctor M. Delorme, also of the veterinary service, who has served in the capacity of Assistant Director.

Numerous news statements concerning the Kindia laboratories have appeared in the American press. Many of them have been seriously misleading because of incompleteness or of confusion of actual status and achievements with plans and projected investigations. Notwithstanding published reports to the contrary, and resulting widespread opinion in America to that effect, the African laboratories have not been the scene of genetic, educational, psychological, and sociological studies of monkeys and apes. Instead, strictly medical inquiries have predominated and only incidentally have contributions been made to other fields of biology. This statement, based on personal observations and the consultation of official reports,[1] is offered to correct the false impression that the Pasteur Institute of Kindia was established primarily for psychobiological research and for the breeding of monkeys and apes in connection therewith.

[1] Calmette, A. Le Laboratoire Pasteur de Kindia (Guinée Française). Bull. Soc. Path. Exot., 1924, xvii, 10–19.

Delorme, M. "Pastoria:" Son histoire et son rôle aujourd'hui et demain. Bull. de liaison entre les Officiers des Réserves de la Guinée et leurs camarades de l'Armée active. Nos. 8 et 9, Février-Mars, 1929.

In 1924, shortly after work had been initiated at "Pastoria"—the convenient popular name for the laboratories—I endeavored to arrange for a visit. It was years, however, before mutually satisfactory conditions were agreed upon and official invitation extended. Finally, during the summer of 1929, accompanied by my daughter, Roberta Watterson Yerkes, who invaluably assisted me as interpreter and observer, I journeyed from Marseilles to Conakry and thence by rail to Kindia. In advance I had conferred with Assistant Director Doctor A. Calmette of the Pasteur Institute of Paris, and with Doctor Wilbert, who also at that time was resident in Paris. With pleasure I acknowledge the courtesies of these gentlemen and most heartily thank them for their generosity in offering me opportunity to visit and observe "Pastoria" and the resources of the laboratories for conduct of research so long as I should be able to stay. To the Colonial officials of French Guinea, and especially to the Governor of the Province, to the Administrator of the circle of Kindia, Mr. de Gentile, and to Doctor Jean Claverie, I am deeply indebted for their cordial reception, hospitality, and material assistance in the fulfillment of my mission. But greatest of all is my obligation to Doctor M. Delorme, who entertained, guided, and instructed us throughout our sojourn, and who spared no pains to satisfy both our needs and our professional desires. Without his devoted and over-generous assistance we should have learned relatively little concerning Pastoria and its surroundings, and enjoyed less. To all of our French friends and colleagues in Paris and Africa who have furthered the objectives of our visit to Pastoria, my daughter and I take this opportunity to express sincere thanks.

Superior to my interest in Africa as home of chimpanzee, gorilla, and many humbler primates, as well as of certain scientifically engaging races of man, rose my professional desire to observe the laboratories at Kindia in their geographical, geological, meteorological, and social setting, to study their equipment, animal resources, personnel, achievements, research and medical services, and above all, their possibilities of development and of coöperation with individuals and institutions the world over. In a sen-

tence I have endeavored to state my purpose and hope in making the long and arduous journey to Kindia. Such description of Pastoria as otherwise would have been introduced at this point belongs rather in the body of the report by Doctor Nissen and there appears.

As background for my prospective observations, I knew that in America, France, Germany, Russia, and possibly elsewhere, certain anthropoid apes and types of monkey were being used with steadily increasing intelligence and effectiveness as subjects of biological inquiry, and I wished especially to discover in what ways and to what extent it might be reasonable to expect that this well-established scientific station in the natural habitat of chimpanzee, baboon, guenon, and several other monkeys, would supplement the important resources of European and American research establishments. In a word, my interest tended to center on inquiry concerning an important phase of the internationalizing of biological research.

In my decision to visit Pastoria and in the distribution of my interest during the visit, undoubtedly I was influenced to an appreciable extent by knowledge that the authorities of the Pasteur Institute of Paris had long hoped for American financial and research assistance in the development and utilization of the laboratories at Kindia. This made it appear not only desirable, but in a measure my duty, to make thorough study of Pastoria, and on the basis of its merits to be prepared to make report and recommendation to interested colleagues and patrons of research. Happily with the assistance of Doctor Delorme I was enabled promptly to gain comprehensive knowledge of the status and history of work at the Station, and with the benefit of his advice to formulate tentatively a plan of coöperation between Pastoria and Yale University which we had every reason to believe might prove possible of realization and valuable in its results. Incidentally, it should be stated that on my return to America I made written report concerning the Pasteur Institute of Kindia, at his request, to Doctor Richard M. Pearce, of the Rockefeller Foundation, for the information of that organization.

Briefly stated, the plan above mentioned, which later was ap-

proved and supported by Doctors Calmette and Wilbert, involved the sending of qualified investigator from the Laboratories of Comparative Psychobiology, Yale University, to Pastoria, in order that he might initiate, with Doctor Delorme's assistance, systematic study of the life of the wild chimpanzee in French Guinea, and at the conclusion of the season of field work, bring back to America a group of young chimpanzees for continued study in our laboratories in New Haven and at Orange Park, Florida. In this plan Pastoria naturally was expected to serve as headquarters and base of operations and its staff as collaborators, while the visiting investigator from Yale should hold himself responsible for the direction, conduct, and costs of field work and the assembling, observation, and transporting of animals for use in America. To Doctor Delorme as well as the writer this seemed an admirably simple, effective way to initiate systematic naturalistic work in the region and to test the practicability and possibilities of coöperation between our respective institutions and staffs.

The significance of what actually happened by reason of my visit, and of the plans which have just been outlined, will be clearer if the organization and program of psychobiological research at Yale University are summarily described, by contrast with what I understand to be the situation in the Russian primate laboratories at Sukhum, Caucasus, on the north shore of the Black Sea.

Yale University is now attempting to further the solution of psychobiological problems, and at the same time to contribute substantially to other fields of biological inquiry, including conspicuously the social and medical, by systematic, sustained, and carefully prearranged use of the anthropoid apes, and more particularly the chimpanzee. The work is being conducted in specially equipped laboratories of Comparative Psychobiology in New Haven and in the Anthropoid Experiment Station at Orange Park, Florida, which is intended primarily for breeding and observation of reproductive cycle and life history. In neither of these establishments is it possible to provide suitable conditions for naturalistic observation. Consequently Yale investigators

must depend upon the resources of Africa, Asia, and adjacent islands, for such reliable account of the traits, habits, diseases, disabilities, social life, of the wild anthropoid as may be obtained only through field studies. The latter, it would appear, may best be pursued from temporary or permanent stations or bases in the life zone of the primate which is to be observed. Obviously Pastoria and French Guinea might serve respectively as scientific base and territory for thoroughgoing naturalistic observation of several of the African primates.

The plan and research program of the recently established Russian primate laboratories at Sukhum[2] differ from those of Yale University most importantly in that the Russian station is expected to serve alike as breeding station and as center for varied biological inquiry. This combination of uses is feasible because of the subtropical climate on the north shore of the Black Sea. Undoubtedly the work of Russian investigators, like our own in America, will more or less insistently demand supplementation by the results of field studies, which necessarily will be made in the natural habitat of the animals instead of in laboratories.

Pastoria appears to be almost ideally located as base for naturalistic study of the chimpanzee. Therefore our lively interest in its resources and in the possibilities of effective coöperation.

Although the Kindia station was founded about 1923 and that of Sukhum in 1927, neither, so far as our present information extends, has published any naturalistic studies. The report for which the present paragraphs are intended as foreword stands as the first field study of the chimpanzee to be made by a professionally trained psychobiologist in pursuance of a program which for its execution clearly enough demands systematic, sustained endeavor, coöperation, and the continuous critical scrutiny and improvement of methods and information. Most earnestly we hope that our colleagues in France and Russia, and indeed throughout the world, may find it alike possible and desirable to join in this proposed thoroughgoing study of the life of the wild anthropoid ape, which would seem to be eminently worthy of

[2] Founded by Dr. J. A. Tobolkin in July, 1927.

effort quite apart from the special value of its results to our
laboratories of psychobiology.

On my return to America from Pastoria in October, 1929, I
immediately took steps to carry into effect the plan which I had
agreed upon with Doctor Delorme. It was heartily approved
and supported alike by the administrative authorities of Yale
University and of the Pasteur Institute of Paris. In December,
1929, Doctor Henry W. Nissen, Assistant Professor of Com-
parative Psychobiology, Yale University, sailed from New York
for Conakry. His mission as member of the staff of our Labora-
tories of Comparative Psychobiology was, as intimated above, to
initiate coöperation with the French at Pastoria, to carry forward
naturalistic study of the chimpanzee, and to bring back with
him a group of the animals. It is his right exclusively to tell the
story of that mission and to present its scientifically valuable
results. But it is my privilege here to state that his difficult and
exacting undertaking was carried through with tact, professional
skill, excellent judgment, and freedom from serious mishap, to a
highly successful and scientifically fruitful conclusion. Those
who are familiar with the West African Coast from Dakar to
Duala will not need to be reminded that Doctor Nissen's expe-
dition was not a vacational trip, to be undertaken light-heartedly
and regardless of risks to health, life, and professional prospects.

ROBERT M. YERKES.

New Haven, Connecticut,
October 9, 1930.

A FIELD STUDY OF THE CHIMPANZEE

OBSERVATIONS OF CHIMPANZEE BEHAVIOR AND ENVIRONMENT IN WESTERN FRENCH GUINEA

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INTRODUCTION

It was early in October, 1929, that Professor Yerkes, then
recently returned from Europe and Africa, first spoke to me of
tentative arrangements which he had made for coöperative rela-
tions between the Laboratories of Comparative Psychobiology of
Yale University and the authorities of the Pasteur Institute,
Paris.   A few weeks later it had been definitely decided that I was
to go to French Guinea as soon as practicable and that I would use
Pastoria, one of the African Laboratories of the Pasteur Institute,
as a base for carrying out my two-fold mission.   One purpose
of my trip was to be the collection of a group of chimpanzees,
suitable for use in the anthropoid research laboratories of Yale
University, and its safe conduct to America.   As time and cir-
cumstances offered, I was to have opportunity for making field
studies of the chimpanzee in his native habitat.   Actually, fortu-
nate circumstances and especially the ready coöperation which I
received from the French Government and from the Pasteur
Institute in obtaining a group of apes, made it possible for me to
spend almost three months at my observational work in the bush.
The principal results of those observations will be presented in the
body of this report.

Professor Yerkes and I considered the advisability of my taking
an assistant or colleague with me.   For two reasons this plan was
abandoned.   Firstly, it seemed improbable that in the few weeks
remaining before my departure we would be able to find a qualified
person available for the trip.   Secondly, it was our expectation
that my field work would be of the nature of a preliminary infor-
mational survey, rather than an expedition which would gather
any great body of factual materials.   We were interested, prima-
rily, in testing the feasibility of field studies and in making a start
at discovering workable methodology and techniques for natural-
istic observation of the chimpanzee.   Such a pioneering effort, we
felt, hardly justified the added expense, responsibilities and
complications involved in doubling the personnel of my
expedition.

I do not feel that the collection of data on living conditions
and behavior of the chimpanzee in his normal or usual environ-

ment stands in any need of justification. If one believes, as an ever increasing number of workers in the biological sciences seem to believe, that laboratory use of this ape is eminently worth-while, then the value of a precise knowledge of the environment in which he has lived and survived for untold generations, and of how he behaves in this favored or favorable environment, should be obvious. This is especially true in the case of comparative psychobiology. We may, for instance, observe modes of behavior in the laboratory which appear to be clear cases of imitation or of the so-called higher mental processes, but which, in the light of previous experiences of the animal in the bush, may turn out to indicate merely the operation of simple memory or of transfer of training mechanisms. Koehler (6) found his chimpanzees very clever in solving Umwege situations. Was this behavior a manifestation of insight or is a simpler explanation possible? More than once in the bush of French Guinea, I saw a group of animals, after stripping a tree of its edible fruit, move on to another tree which offered more food. Often this second tree was visible from the branches of the first one. Let us designate the two trees as A and B, respectively. The first impulse, probably, would be to move directly from the branches of tree A to those of tree B. That movement was impossible. The animal descended to the ground and then, guided either by direct vision of B or by some other directional cue, walked to B and ascended. Such a situation must arise often in the life of a wild chimpanzee. Obviously it affords excellent opportunity for learning, by experience, correct adaptation to the Umwege situation. Transfer of the habit from vertical to horizontal arrangement would seem to be the only really new element presented in Koehler's experimental situation. The genesis of behavior exhibited in many stick experiments undoubtedly is to be found in experiences which the animals have in the bush. In feeding and in climbing around in trees, branches are *often* broken off. Later, these may be used in play, and various uses to which a stick can be put may be discovered accidentally. These examples may suffice to show how deceptively easy it is to assign some superior faculty, such as insight, to animals

which are merely exercising habits long since established by the
relatively simple process of trial and error learning.   On the other
hand, we should never be able to posit limits for infra-human
capacities without knowing how these creatures act in an environ-
ment which, so far as we know, may be infinitely more favorable
for the development and expression of such capacities than is a
laboratory setting.   It is probably a safe guess that social rela-
tions, including especially sex behavior, are profoundly different
in the bush than in the laboratory, but until we have a picture of
what actually goes on in the field, such differences are merely
conjectures, giving no basis for sound interpretation or explana-
tion.   Even if we study animals born in the laboratory, whose
ontogenetic histories are on record in all detail, we certainly must
know a great deal about their phylogeny before we can interpret
our experimental findings with assurance and comprehensiveness.
We may say, then, that field observations alone can supply
the comparative psychobiologist with certain indispensable
supplementation of his experimental work.   That such observa-
tions may throw light also on problems less specific to this branch
of science, is a possibility which it would be out of place to discuss
here.

The recent encyclopedic survey of the literature on anthropoid
apes by Yerkes and Yerkes (9) includes a rather complete sum-
mary of those observations made in the field of chimpanzee be-
havior which possess a reasonable degree of importance and
credibility.   This work renders a review of the literature, here,
superfluous.   In discussing my findings in detail, only brief and
occasional mention will be made of the related observations of
others.   Such brevity or omission of reference is justified only by
the knowledge that the interested reader can find in "The Great
Apes" a far more comprehensive treatment of earlier work than
possibly could be presented in this place.   The contributions of
Aschemeier 1921, 1922, Barns 1922, Barns 1923, Bartlett 1885,
Brehm 1922, Buck 1927, Burton 1876, Christy 1915, Du Chaillu
1861, Falkenstein 1879, Forbes 1894, Garner 1896, 1919, Heck
1922, Honore 1927, Jenks 1911, Jennison 1915, Jones 1916, Kear-
ton 1925, Koehler 1925, von Koppenfels 1877, Livingstone 1875,

Martin 1841, Mathews 1788, von Oertzen 1913, Pechuel-Loesche 1882, Reade 1864, Reichart 1884, Reichenow 1920, Rennie 1838, Rothmann and Teuber 1915, Savage and Wyman 1843–44, Sayers 1839, Schweinfurth 1874, Sokolowsky 1908, 1915, Sonntag 1923, 1924, Yerkes 1925, and Yerkes and Learned 1925, are among those of outstanding importance or of special significance in relation to the present study.

Although mine was a one-man expedition, the number of those who contributed, directly or indirectly, to the success of my work, is large. To all of these, whether or not their contributions find specific mention here, I wish to express my heartfelt appreciation and warm thanks.

Quite obviously, my trip was possible only because of Professor Yerkes' previous conferences and arrangements with the authorities of the Pasteur Institute at Paris and at Kindia (French Guinea). I am deeply grateful to Professor Yerkes, both for entrusting to me the responsibility of initiating the realization of his long-cherished plans, and for the wealth of suggestions, stimulation, and moral encouragement which he gave me so generously. To Doctor and Mrs. Yerkes I wish to express sincere appreciation also for their critical reading of the manuscript and for many acts of friendship which contributed to my peace of mind while in Africa.

During my stay at Pastoria, Doctor M. Delorme was in charge of this outpost of the Pasteur Institute of Paris, Doctor Wilbert being in France at the time. From conversation with Professor Yerkes, I was prepared to find, in Doctor Delorme, a charming host and a willing, valuable helper. My optimistic expectations were far exceeded. Doctor Delorme had a comprehensive understanding of my intended work and of my needs, and he spared neither time nor energy in smoothing the way for me. His advice, based on wide experience in tropical Africa, was invaluable. His help, in securing for me three native assistants and in enlisting the coöperation of several Colonial government officials, contributed importantly to satisfactory working conditions and to my relative comfort in the bush. But my greatest debt of gratitude to Doctor Delorme, I feel, is for the welcome and friendship which he ex-

tended to me.   Those who have worked in tropical Africa, isolated for long periods of time from all companionship save that of the natives, will understand how much this friendship meant to me. If it were not for the fact that I must take full responsibility for the field observations recorded in the following pages, I should have insisted that Doctor Delorme appear as collaborator of this report.

I did not have the pleasure of meeting Doctor Calmette of the Pasteur Institute, Paris, but am fully aware that he, as well as Doctor Wilbert, was responsible for many of the courtesies shown me at Conakry and at Kindia.   Indeed, without the coöperation of Doctors Calmette and Wilbert, enlisted by Professor Yerkes, my work under such favorable conditions would have been out of the question.

The Colonial officials of the French Government were, without exception, generously coöperative.   I wish to mention especially the following: the Governor of the French West African Colonies; the Governor of French Guinea; Colonel Berthomé, Military Commandant of French Guinea; M. de Gentile, Administrator of the Cercle de Kindia, and his assistant, M. van Ormelingen.   I cannot forego voicing my thanks also to those who extended cordial hospitality at Kindia: Colonel and Mrs. Berthomé, Mr. and Mrs. de Gentile, Captain and Mrs. de Perthuis, Doctor and Mrs. Jouhaud, Mr. and Mrs. van Ormelingen, Mr. and Mrs. Armstrong, Mr. and Mrs. Carrière, and Mr. and Mrs. Entwisle.

In various ways I am indebted to Doctors Harold J. Coolidge, Jr. and George C. Shattuck, of Harvard University, and to Doctor O. L. Tinklepaugh of Yale University.   To Doctors C. Burn, A. B. Dayton and W. G. Downs, Jr. of the Yale School of Medicine and especially to Doctor Joseph De Vita of New Haven, thanks are due for unselfish and efficient assistance in helping the animals, which I brought to New Haven, through their first difficult winter in a temperate climate.

Pastoria, especially in the person of Doctor Delorme, played an important part in both of the principal divisions of my work.   A brief description of the physical aspects of the Laboratory and of its activities is therefore pertinent.   For a more complete account

we may refer to the articles by Doctors Calmette (3) and Delorme (4) already mentioned by Professor Yerkes in the Foreword. The location of Pastoria is ideal in many ways. Sufficiently isolated to insure undisturbed activity and possibility of unlimited expansion, it is nevertheless easily accessible to Kindia by a fair road. Kindia, where most of the necessities and some of the luxuries of life may be purchased, is connected with the Coast by rail. On an elevation in the midst of pleasantly rolling country, Pastoria enjoys most of the breezes that blow. Nearby springs afford a good all-year water supply. The grounds, covering an area of 35 hectares (4), are laid out practically as well as artistically. Irrigated gardens furnish a variety of vegetables and fruits, especially, of course, during the dry season. Native and imported flowers and trees in profusion are skillfully arranged to make Pastoria a spot of beauty in its setting of African wilderness.

The principal building is the only one of the group of Pastoria structures which is considered permanent; all the others are regarded as temporary and provisional. It is largely of concrete construction and has two floors or stories. Most of the ground floor is devoted to the main laboratory, extremely well equipped, it seemed to me, especially for bacteriological work. In addition, there are two offices, a storage room for laboratory supplies, a small dark room and three or four other rooms serving miscellaneous purposes. The second floor of this building furnishes the living quarters for the Director of the Laboratory. There are two large bedrooms, living and dining rooms, bath, toilet, and kitchen. This building is equipped with electric light, generated on the premises, and with running water.

Animals are quartered chiefly in three buildings. One of these is devoted exclusively to non-primates, especially rabbits and guinea pigs. A second, double building, houses chimpanzees and the larger monkeys, especially baboons. Approximately 50 two-room "apartments" are available here. Between the two sections of this structure, is a building containing a few animals (rats, mice, and others) and a large room used for autopsies and for treating animals which cannot conveniently be brought to the main laboratory. At one end of this building there is provision

for keeping a few larger animals (cattle) and dogs. The third quarters building houses the smaller monkeys and young chimpanzees.

A number of auxiliary buildings are present, among which should be mentioned especially the wood shop, storage building, crematory, garage, and a service building containing the electrical generating plant, water pump, several power tools and a refrigeration apparatus and room. A small residence is provided for the European mechanician, until recently the only white assistant to the Director. During my stay at Pastoria a second young European was added to the staff to help with clerical duties attendant upon the management of this large enterprise. Some forty or more natives are constantly employed, many of these living in huts within the confines of Pastoria.

The Pasteur Institute of Kindia, as its parent organization at Paris, serves practical medical as well as purely scientific ends. It has supplied quantities of vaccines and serums to the public health services of French Guinea and other African colonies (4). It has further promoted health in the Colony by making large numbers of bacteriological analyses. During my stay at Pastoria, Doctor Delorme developed what promises to be a fruitful method for combating the hordes of locusts which have recently invaded the country. He was also working on the diagnosis and treatment of a disease attacking banana plants on some of the nearby plantations. Research activities have been varied, although all of them, I believe, have been in the field of pathology. Doctor Wilbert conducted early tests of vaccination against tuberculosis by the method of Calmette and Guérin, using primates as subjects. Various studies of experimental cancer, anthrax, rabies, malaria, the pneumonias, encephalitis and other diseases have been undertaken. Chimpanzees are readily available to Pastoria in relatively large numbers. Until recently, a discouragingly large percentage of animals received at the Laboratory died of a mysterious, rapidly acting ailment, termed by Doctor Delorme, the chimpanzee "Peste." Dr. Delorme worked long at the identification of this disease and quite recently developed an adequate preventive treatment. Tests of his method were conducted early

in 1930 and gave highly satisfactory results. The details of this achievement—and certainly it is an achievement of consequence to save countless numbers of these uniquely valuable animals for experimental work— will appear soon, I presume, in one of the French journals.

Pastoria occupies an enviable position in being located within easy accessibility of almost unlimited supplies of monkeys, chimpanzees, and many other tropical animals, and yet being in a country which does not place the health of European or American scientists working there in unreasonable jeopardy. That the potentialities of the Station have not been taken advantage of more fully is probably attributable to lack of funds. For some time there has been urgent need of additional housing space to take care of more investigators; such living quarters are now being definitely planned for, I understand. It is to be hoped that the unparalleled possibilities of Pastoria for medical science, as well as for broadly psychobiological study, may not be allowed to remain unexploited for long, but that they may soon be turned to solid achievement.

1. PREPARATIONS; CONDITIONS AND METHODS OF WORK

Preparations for the trip to Africa were necessarily hurried, only two months being available for this work. There would be little profit in my giving here, a detailed description of my equipment, or in stating critically in what respects this equipment, both physical and otherwise, proved to be adequate or inadequate in the light of subsequent experience. There are others who can speak on general expeditionary requirements with more authority than can I. Suffice it to say, that although in some minor details I was under- over- or ill-equipped, I was not embarrassed by any serious lacks. On December 26, 1929, I sailed from New York on one of the American-West African Line boats, landing at Conakry, French Guinea, January 22, 1930. Several days were spent at Conakry in attending to official duties, and on January 25th I travelled by train to Kindia, about 125 kilometers inland. There I was greeted by Dr. Delorme who brought me to Pastoria, about seven kilometers north of Kindia. Pastoria served me as

a general base during most of my stay in Africa, although I actually lived there only a fraction of the time. The temporal and geographical distribution of my activities may be outlined for convenient reference as follows:

> December 26, 1929, left New York.
> January 22 to 24, 1930, at Conakry.
> January 25 to February 5, at Pastoria.
> February 6 to March 10, in the field.
> March 11 to March 16, at Pastoria.
> March 17 to March 27, in the field.
> March 28 to April 1, at Pastoria.
> April 2 to May 5, in the field.
> May 6 to May 26, at Pastoria.
> May 27 to 29, at Conakry.
> June 17, 1930, arrived in New York.

The first period spent at Pastoria was a busy one. In addition to partially acclimatizing myself and completing preparations for field work, I made preliminary arrangements towards obtaining a group of chimpanzees for the Yale Laboratories of Comparative Psychobiology. The next two visits at Pastoria were less strenuous and provided pleasant relaxation from the strain of field work. The final period at my general base (in May) again presented a variety of tasks: the care and observation of Kambi, an infant chimpanzee captured in the bush, and of Bimba, another infant ape, purchased from a native who had shot the mother; administration of a battery of performance tests to fifty native children; lastly, preparations for transporting sixteen chimpanzees (plus the two baby animals which I had procured independently) captured for me by natives at the instigation of officials of the French Government and of the Pasteur Institute. Most of the chimpanzees which I brought back to America have since commenced their services as subjects in various psychobiological investigations.

Final preparations completed, I started field work February 6th. The native village of Nérébili, comprising about 25 huts and located some 20 kilometers east of Kindia in what was reported to be good chimpanzee country, was selected as my first

field base.   The region proved well suited to my purposes, and I
continued to use Nérébili as my chief camp in the field for the rest
of February and all of March.   Early in April I removed my sup-
plies and equipment to Kambi, a smaller native village, about 8
kilometers further east.   My activities in the bush were confined
to an area of perhaps 20 kilometers in diameter with its center a
little east of Kambi.   This geographical concentration of my work
had certain advantages.   It enabled me to keep in fairly close
touch with Pastoria and to return there promptly when necessary;
in view of the unpredictable length of time that I would be able
to spend in the bush, and my uncertainty as to just how arrange-
ments for collection of a group of animals for export would mate-
rialize, this appeared desirable.   Furthermore, by keeping within
this relatively small area, which included a wide diversity of topo-
graphical features, I became rather well acquainted with the terri-
tory and probably learned more about the differential effects of
various environmental factors on chimpanzee behavior than
if I had constantly invaded new and unfamiliar country.   It is
entirely possible that a wider spatial range of operations might
have opened up better opportunities for observation, but this is
doubtful.   Chimpanzees are occasionally captured in the section
where I was working, it is true, but that is the case in other parts
of French Guinea as well.   Further inland, I understand, they are
also shot for their meat by the natives.   Especially because of the
limited time at my disposal, concentration of effort seemed desir-
able.   For an expedition of longer duration a survey of wider
territories is certainly to be recommended.

The twelve months of the year are divided into two sharply dis-
tinct seasons in French Guinea.   During the wet season, extend-
ing roughly from early June to late October, it rains almost con-
tinually, whereas rain during the dry season, from November to
May, except for occasional showers in the transition months, is a
great rarity.   My observations, it will be noted, were made dur-
ing the latter part of the dry season; there were less than 12 hours
of rain while I was in the bush.   Humidity is high, even in the
dry season; the relative humidity rarely falls below 50 and often
rises to 90 or more (5).   The difficulties which this high moisture

content of the air presents in photography and in preserving certain types of equipment and supplies are obvious. The temperature ranges from about 20°C. before sunrise to 38° or 39° in the afternoon. Compared with temperatures found in certain other parts of Africa this is not excessive, but in combination with the high humidity found in French Guinea, the heat is oppressive and fatiguing. Constant precaution against sunstroke is necessary. The natives do not seem to mind the heat greatly, not as much, apparently, as do chimpanzees.

The district where I worked is hilly. It contains many spring-fed brooks or creeks, some of which end their courses within 50 or 100 feet from the sources while others flow on for many miles. Most of these streams are narrow and shallow enough to be waded; they are called "marigots" by the Colonists, a term which was adopted in my notes. The only river of any size in this region is near its eastern boundary, the Kolenté. Vegetation is extremely rich wherever there is water; here towering trees and thick underbush of vines and grasses give a picture conforming to the traditional idea of a jungle. The hillsides and uplands are often covered with more or less dense forests of smaller trees, although many areas are found bare of all vegetation except bush grass or bamboo, which is almost everywhere. This grass is dense and grows 5 to 10 feet high. In the dry season it becomes dry, brown, and brittle; near villages large fields of it are often burned down in preparation for planting rice and cassava.

The bush abounds in wild life of many kinds. Especially numerous are insects and birds. The panther, wild-cat, buffalo, serpent, crocodile, hippopotamus, porcupine and other rodents, deer, antelope, and wild boar are fairly common. There are no lions or elephants in this section. Baboons, travelling in bands of one hundred or more, were encountered frequently. The baboon, in contrast to all other primates seen in the bush, showed no fear of man and at times even manifested an inclination to attack. Several kinds of tree-dwelling monkeys were seen in numbers almost every day. Chimpanzees also, as described below, are numerous in this section.

French Guinea contains about two million native inhabitants

within its 95,218 square miles. This is somewhat more than twice the population of the State of Oregon, which has approximately the same area. In the section under consideration, most of the natives belong to the Sousou tribe The Foula tribe is represented in settlements scattered through the region. Both Sousous and Foulas are members of the Mandingon race and both belong to the Mohammedan faith. In many other respects they are very different. The Sousou adapts well temperamentally to working with and for white men, is relatively honest and loyal but rather lazy and stupid. The Foula, on the other hand, does not like to take orders from white men, is a hard and intelligent worker, but has a reputation for being treacherous and dishonest. From Kindia and Pastoria I brought three servants: Ben, a Sousou who spoke a little English, having come from Sierra Leone; Bokari, also Sousou, who, as member of the native police was detailed to guard my belongings, and Moussa, member of the Thoma tribe, who had come from the eastern part of French Guinea. Moussa was an exceptionally intelligent and able native; his services were worth those of a dozen others. At the Sousou villages of Nérébili and Kambi I hired 6 porters, guides and scouts, who were with me throughout the period of my work. For special purposes I occasionally had as many as forty Sousou natives working for me.

Methods and techniques for observing the wild chimpanzee in his native habitat were developed gradually. In general they are time-honored modes of naturalistic work, containing no radically novel elements, but adapted to meet the particular habits of these apes and the special conditions of their environment.

The use of blinds was tried without success three times. Twice I stayed in blinds, ingeniously constructed by native assistants in favorable places, for an entire day without seeing an ape. Two factors are probably responsible for the failure of this method: first, the ease with which sharp-sighted chimpanzees discover artificial abnormalities in the bush and their timidity in approaching strange or fearful objects; second, the unpredictable locus of the future wanderings of a group of animals—a perfect blind may be constructed where the apes have been for several days, but for

known or unknown reasons they may not appear there again for a week or a month. With a larger personnel and more time the method might prove fruitful of unusually valuable observations. The use of lures, or the presentation of chimpanzee delicacies at the same spot over long periods of time might be feasible, especially if employed in conjunction with a good blind and perhaps with a few tame animals, but largely because of lack of time I did not try out this plan. It is relatively easy to surround a group of animals with a circle of many natives and then to observe the apes under these artificial conditions for a day or even longer. I tried this a few times, mainly in order to get absolutely reliable data on the age and sex composition of a group; these attempts, however, met with failure because of the inexperience and stupidity of my Sousou helpers. Trailing, in the ordinary sense of the term, is not easy. The ground is usually too hard to bear visible footprints, and paths through the grass or underbrush which lead in all directions cannot always be differentiated as to age. On the other hand, feces, fresh peelings of fruit, newly broken tree branches and other similar signs of the recent presence of apes, frequently assisted in trailing them. The best indications, however, were given by the apes themselves, in the form of cries and drumming which could be heard for considerable distances. Once the approximate position of a group of animals had been ascertained, the greatest precautions against letting them see us had to be observed. They are extremely timid, especially, it seems, when one shows interest in them. Their ability to detect approaching danger appears to lie especially in excellent vision; in my work I was impressed by their apparent obtuseness to slight noises and odors. In order to minimize the possibility of detection as far as possible, I often left my porters and guides at a safe distance, and then attempted to get within range of observation myself. If a group was on the move I could sometimes anticipate where they would come and secrete myself there beforehand. When this technique worked, it had obvious advantages. Much of my work was done with seven-magnification binoculars, which permitted good observation at relatively great distances. Sometimes I was as close as fifteen or twenty feet from the ani-

mals, but most sustained observations were from greater distances: fifty to five hundred feet. In order to save time, I usually sent out scouts to locate the sleeping place of a group. The next morning I was at the place before break of day and would then attempt to keep the animals under observation as long as possible. If they escaped us completely, I again sent out scouts or accompanied my guides in a more or less undirected attempt to find the same or another group. At night I usually returned to my field base, although if the distance was great I camped in a nearer village. Twice I spent the entire night close to where a group of animals was sleeping.

Photographic equipment, including one or two still picture cameras and a small cinematic machine, was always taken along in the field but was used relatively infrequently. When opportunity for photography was good, the opportunity for making important observations was obviously also good. I felt, as did Dr. Yerkes, who advised me in the matter, that accurate observation and subsequent verbal description were of much greater scientific value than the taking of pictures. As intimated above, the animals are apparently very keen visually. The movements necessary to focus and further adjust a camera often would have scared a group away which otherwise could be observed quietly for some time. It is clear, furthermore, that one person cannot observe well and manipulate a camera at the same time. I attempted to train Moussa, my most intelligent assistant, in the use of one of the simpler cameras, but his efforts met with no great success. Photographic conditions, also, were unfavorable. The darkly pigmented chimpanzee requires much light to register on a sensitive film, and under trees of forest or jungle, illumination, especially in this humid climate, is not good. This necessitated a wide aperture and long exposure, with attendant shallow depth of focus and blurring when there was movement. (In some of the pictures which I thought would turn out best, the animals are indistinguishable from splotches of dark shadow.) Animals in trees, silhouetted against the sky, naturally gave pictures showing much contrast and little detail.

A net period of seventy-eight days was spent in the bush (page

10). Active field work occupied sixty-four days, the other fourteen days being devoted to rest, keeping equipment in condition, writing up my notes, and other similar duties. On fifteen days I either was unsuccessful in finding animals or was engaged in making studies of nests or other features of the environment. One or more groups of animals were seen on forty-nine different days. On five of these I saw what were clearly two distinct groups of apes, and on one day three separate groups. Seven times I saw animals twice or more during the same day without being certain whether the later group was the same one seen earlier or a different one. The total of from fifty-six to sixty-three groups of animals observed does not, of course, indicate that many distinct chimpanzee organizations. Certainly in some cases and probably in many others, the same band was seen on more than one day. My guess—and it cannot be much more than a guess—is that the total number of different groups seen during the period of my work did not exceed twenty or twenty-five. I should further guess that there were, on the average, two or three groups at a time within the area where I was active.

The most complete account of my observations would probably be given by a presentation of all of my original notes. Such a procedure would expand this report to unwarranted proportions and would leave the material in rather inconvenient form. A mere digest of the results, on the other hand, could not impart certain important but non-explicit aspects of the work and might well give an inadequate or even wrong impression. In order to make the findings readable and usable, and yet to preserve something of the picture as a whole, a compromise method of presentation is adopted: under each one of several more or less logically determined categories, are summarized all observations pertaining to it. Quotations from original notes illustrate and supplement the summaries; for the sake of continuity and clarity such quotations are often fairly lengthy. They have not been edited, since being written in the field, except as to orthography and grammar.

2. SOCIAL ORGANIZATION AND PHYSICAL APPEARANCE

Practically all of the accounts which have come to us regarding free life of the chimpanzee agree that these animals are highly

socialized and that they almost invariably live in bands or groups (9, p. 246–247). This conclusion the present study fully confirms. Some writers have applied the term "family" to such organizations; others, more critically minded, have called them simply "groups" or "bands." I doubt that sufficient evidence is yet at hand to warrant the assertion that any one certain factor is responsible for the colligating of these apes, and therefore feel that the designation, "family," is at present gratuitous. The difficulty of determining relationships within the group will become apparent as we proceed in our account.

In many cases, the number of animals in a group can be estimated with reasonable accuracy. The individuals composing it do not frequently stray very far apart. (Evidence that two or more groups sometimes combine temporarily or that sections of the same group separate for longer periods, is discussed below.) Although in at least fifty per cent of my observations I probably did not actually see every individual belonging to the group, collateral evidence, furnished by nests, feces, vocalization, and movements of trees or bushes, often assisted in reaching a close approximation to the total number of animals. Numerous instances are recorded in my notes of seeing one or several animals, without my having reliable clue as to whether the animals seen were the only ones in the vicinity or whether there were more near by. Such cases are not included in my estimates of the size of groups.

There were twenty-five observations which, I feel, allowed fairly accurate count of the total number of animals in a group; the lowest number is four, the highest is fourteen, and the average is 8.5. Two instances of apparent temporary combining of groups gave totals of sixteen and eighteen, respectively. Natives told me that the groups are much larger during the rainy season; this statement, of course, I was not able to verify.

Regarding the stability and permanence of groups, my observations allow of no conclusions. Several times I saw the members of two groups (or perhaps they were halves of the same group) mingle, giving excellent opportunity for re-organization. But I was never in position to ascertain with certainty whether or not such changes in grouping had taken place.

It is gospel among the natives that a chimpanzee group consists of the male leader, his wives and his children. The family of the French Guinea negro is thus constituted, which makes it probable that his observations of the apes are strongly tainted by anthropomorphic interpretation. In six instances I was able to determine that there were at least two mature males present in a group. These observations certainly do not exclude the possibility that a chimpanzee band is composed of a polygamous male and his family, since a male may well have one or more mature sons, but they do lend some probability to alternative explanations. The extreme possibility, that neither male nor female chimpanzee is limited to a single mate, even temporarily, has never been satisfactorily disproven, as far as I know. Presence of more than one mature female in a group was established in nine cases. I was able to determine the sex of only about 25 per cent of the animals observed; such determination was made on the basis of sight of the sex organs. Other features, such as breasts, face "color," shape of head, and width of shoulders or of hips, were found to be unreliable in indicating sex. Of this number, 65 per cent were females, 35 per cent were males. In this connection it should be noted that the large majority of animals received at Pastoria, 75 per cent or more I believe, are females. This suggests a birth ratio very much more uneven than that found in human races, or else an even higher death rate among infant males than in human races. Not to be overlooked in this connection, however, is the possibility that certain factors may operate to produce selection, according to sex, in samples of observed and captured animals.

Although, as has been said, my observations confirm the traditional belief that the chimpanzee likes company and almost always lives in bands or groups, there is some evidence that certain individuals, temporarily at least, lead hermit lives. Among natives of French Guinea the belief is prevalent that old and feeble males are ostracized from the group and thereafter live in isolation from their fellows. Although on one occasion I did see a mature male without being able to find signs of any other animals in the vicinity, conditions were such that I could not be sure this

was really a hermit animal.   Natives also told me that females isolate themselves from the group a few days before parturition and do not rejoin it until two weeks or more later.   One most exceptional observation, made on March 20, might be interpreted as supporting this statement.   The observation was unique and interesting in several respects so I quote somewhat fully from original notes.

March 20.   Started at 5 A.M. going NE . . . . At 10:30 I stopped to rest and sent the porters out to scout.   They returned at 10:50, reporting having sighted *one* chimpanzee.   Said that when they first saw this animal it was on the branch of a small tree, only 8 or 10 feet from the ground.   This branch broke under the animal's weight, and the chimp at once ascended to where I first saw it, near the top of a giant Fromager. This tree was in a shallow and narrow marigot valley, heavily wooded and shaded, especially by tall trees.   The stream is fairly wide (15 feet), sluggish, not deep.   It was 11 A.M. when I first saw the animal, a large female (sex without doubt).   The marigot flows W to E.   The animal stayed in this same tree until 2:35, with movements as recorded below.   The genital area was enormous, swollen, reddish pink.   Vaginal orifice distinguishable, seemed unusually large.   Long black hair which seemed to stick out from the body, especially from arms.   Face black. Ears not prominent.   Estimated the genital area to be too large to be covered by one of the chimp's hands.   Head seemed very broad; it was not bald.   Generally sat with the right hand holding to a higher limb, legs doubled up so that genital area was visible under the branch on which the animal sat.   Really one should say that the animal squatted (instead of "sat"), for its weight was on feet or legs and was partly suspended, I believe, from the one arm.   The free arm did not function in supporting the chimp in this position; it was usually folded in its "lap."   There were 3 series of cries between 11 and 12 o'clock, an especially loud and long one at 11:35.   Usually there was one long, low-pitched grunting cry, very loud, followed by two short, higher pitched "panting" cries; each such group of sounds was followed by a short pause, and then another group.   What I have termed a series of cries consisted of ten to fourteen of such three-note groups.   The first note was a regular roar which, I thought, had a touch of fear and supplication in it.   The mouth was wide open during this cry—not very funnel-shaped.   After each 3-note group, as I have said, there was a short pause; during this pause could be heard, very distinctly, the echo of the

sounds just made. . Did the animal think it was being answered, or did
it like to hear its own echo?   Conceivable that some of these cries were
a sort of play, but certainly the later ones (see below) were more than
that, being accompanied by unmistakable signs of emotional involve-
ment.   During all or most of these vocalizations the animal was either
looking directly at us (was it trying to intimidate, defy us?) or slightly
to the W.   Sometimes there were sounds in the woods, made by mon-
keys, birds and by my party; the chimp invariably looked at us but often
looked in other directions, briefly, *as if* looking for, expecting, or hearing
something.   Part of the time, when looking at us, the chimp had her
head cocked to one side in a manner which we would call wistful.
Climbed up a little and then down again at 11:13.   At first she seemed
to be trying to hide behind the trunk of the Fromager, but not so later.
At 12:10 became a little interested in playing with the blossoms of the
Fromager (the pods have opened up, exposing the "cotton").   At 12:15
lay down on her side, still hanging on above with right arm (lay on left
side).   But did not maintain this position long, the noise produced when
I changed the settings on Graflex evidently alarming her.   Except for
the first 40 minutes of observation one could not say that the chimp
expressed fear or great anxiety in her behavior.   It was more an attitude
of watchful waiting.   At 12:30 the chimp was literally embracing trunk
of tree.   Opened mouth very wide, but without sound, and then gave a
good yawn.   A second "double" yawn was given 2 minutes later.   I
am not sure that the animal never sat on the genital area, but I never
saw her do so.   Generally a squatting position.   Occasionally the animal
let go, with right arm, in order to scratch herself, and then her balance
seemed very precarious indeed. . . . . At 1 P.M. I approached the
tree, and as I was again withdrawing from it the chimp gave a series of
cries—not the "panting" cry, but disconnected roars, very loud.   This
vocalization was accompanied by a quick up and down movement
(bending at knees).   What sort of emotion this expressed is, of course,
conjectural, but that there was emotion there is no doubt. . . . . A few
minutes later I had Moma approach the tree as I had done, and then
there was a repetition of the performance, plus the breaking of one
or more Fromager pods—whether accidental or voluntary the pictures
may show. . . . . Then all of us moved around the tree, and the animal
changed her position a number of times. . . . . At 1:40 the chimp was
reclining close to the tree trunk.   At 2 P.M. I was taking pictures with
Graflex when quite suddenly the animal travelled out on a branch.   She
was evidently making up her mind to cross over to an adjacent tree; this

meant a short jump. A minute or so before she jumped she let out a series of yells (same as at 1 P.M.), at the same time bending rapidly at knees about 6 times. The tree to which she jumped, also very high, had thick foliage (not a Fromager), making observation and photography much more difficult. In this tree the animal remained until our departure half an hour after sunset—about 6:30. At first she moved about a bit but after half an hour in this tree she found a comfortable place near the top where she was still resting when we last saw her. During this time the chimp regarded us more or less persistently, even when she was lying on her back in crotch of tree. There was no sound production after 2:30. At 4 P.M. all of my party secreted themselves well under some bushes; then I sent 4 of my porters home (film all used up). Moma and I remained in our hiding place, perfectly quiet, for 30 minutes. This had no effect on behavior of the chimp.

While in the Fromager, the animal was some 150 feet above the ground. We were not much further than that away from her, for although we were perhaps 75 feet from the the base of the tree, we were also on higher ground than where the Fromager took root. The chimp did not eat during the 7-hour period of observation, but often scratched herself and then put hand to mouth. Scratching of back was usually with right hand over left shoulder. In travelling on branch, it was always on all fours. It seems very curious indeed why the animal did not try to escape or descend; position of trees seemed to make it an easy matter to do either. Moma says she feared to descend, suspecting a trap. Possible, of course, that this animal once escaped from a capturing party and learned that it is safer to remain high up than to descend. All my other helpers agree on this theory: Animal gave birth to a baby a short (?) time ago. When danger approached, she hid baby on ground and then ascended in order to keep watch over the infant. The following day, although we arrived before daybreak, we were unable to find this most exceptional animal again.

It is very difficult indeed to judge of the ages of chimpanzees, especially when seen at a distance in the bush where there are no familiar objects against which one can measure them. In general I was impressed with the large proportion of young animals, "babies" and "children." This impression *may* be based upon an illusion. Of this much, however, I am quite certain: If the size of the largest animal in a group is represented by X, then the size of about fifty per cent of the rest of the animals in the group

was $1/2 X$ or less.   Some individuals seen in the bush seemed extremely large, but since actual measurement was impossible,
nothing definite can be said regarding the dimensions of such
animals.   That there is no immediate danger of the chimpanzee
disappearing from the African bush is suggested not only by the
large number of these animals found over wide areas of Africa,
but also by the fact that I saw infants, too small to walk by themselves, in twenty-five per cent of the groups observed.   In one
instance, there were two infants, each carried by an adult, in a
group.

Among the many individual differences noted in physical appearance, few are more obvious than those relating to face "color."
A number of animals were found which could be definitely termed
"black face," but relatively few exactly fitted the description
"white face."   Intermediate shadings, found in most animals,
were extremely varied.   Several matters of interest may be noted
in this connection.   We shall designate as "white-faced" those
animals not completely, or nearly completely, black.   (1) White
faces were in the majority, constituting about seventy-five per cent
of animals observed.   (2) In the same group in which there were
one or more black faces, there were also white faces.   (3) Face
"color" does not appear to be closely correlated with either age or
sex, although most of the black faces seen were mature females.
Two of the black faces, however, were mature males and one was
an immature male, about four years old.   White faces were found
among young and old, male and female animals.   (4) A black-
faced female was seen with a white-faced infant clinging to her.
Presumably she was the mother of the smaller animal.   These
facts suggest that face "color" is not a species, or subspecies,
differentiating characteristic, or, if it was that originally, that the
types successfully interbreed.   (Compare 9, p. 203–216.)

The hair of the animals was usually black, except that on parts
of the face and around the ano-genital region there were white
hairs.   On one occasion I thought that I saw an animal with light
brown or orange-colored hair.   The observation was brief and I
concluded later that the fact that the animal had been almost
directly between me and the sun, perhaps had been responsible for

an optical illusion.   I mention the occurrence, because at Pastoria I was told that a captured animal, brought there several years before, had exhibited this same curious and exceptional coloration.[1]   I neither saw nor heard of albino chimpanzees.

Most of the animals which came under my observation appeared to be in good health.   A number of the females seemed a little lean, while only three animals (two males, one female) are described in my notes as being fat.   There were no cripples, but there were some indications of disease or of extreme old age.

April 16.   One animal was close by—unobstructed view.   It was a very large and rather fat female.   Moved slowly and somewhat clumsily. Several times she looked exactly in our direction but gave no signs of having noticed anything unusual.   She was eating fruit of the Gerenyi tree; sometimes she stripped off a number from branch or twig and brought food to mouth with hand, and sometimes she brought a twig close to her mouth and then ate directly, twig to mouth.   There were no signs of chewing.   A black-face, but her lower jaw seemed exceptionally white.   Ears small.   Genital area projected like a stalk; there was no swelling or redness.   This animal either had gray or white hairs on the lower half of her back and on the legs around knees, or else she had white skin and was bare of hair at these places.   Believe the former was the case.

April 15.   A little later, when it was lighter, I could see a number of animals feeding in a Naray tree.   There were from 6 to 8 animals in the tree, but I had more than fleeting glimpses of only 3.   One of these was a good-sized but somewhat emaciated female.   Her buttocks projected most conspicuously and almost looked like a short thick tail.   Definitely not in period of menstrual swelling.   On her back were 4 or 5 white spots which looked like sores.   Her neck was conspicuous, whereas one usually hardly realizes that an adult chimp has a neck.   Altogether the animal did not look "typical."   The head seemed small and odd-shaped. Dark faced—not black faced.

That the chimpanzee occasionally suffers from respiratory troubles, even in his native habitat, appears from the following. A group of about seven animals was under observation from 3 to 5 P.M.

[1] See also Yerkes and Yerkes, p. 201.

March 5. During this period I heard, at frequent intervals, sounds as of a man with cold in chest; coughing and also (twice) sneezing. Moyla said these sounds came from a chimp. I know that were there no natives in the area under observation. The sound was similar to that made by the animals in New Haven when we were sulphurizing the building.

Two days later we were at the same place, early in the morning. I cannot be sure that the animals were the same ones observed on the 5th.

March 7. Was established in what seemed to be a favorable place for observation, in some bushes, by 5:45. Cameras ready. At 5:50 heard chimp cries, low-pitched, almost like a grunt. At 6 A.M. the sun became visible—a saturated reddish orange color. At about this time could hear chimps moving in trees, branches swishing, but could not see them. At 6:10 there was a short fear-pain cry, accompanied by grunts. One chimp was sneezing—not coughing. Up to 6:30 there was much cracking of branches, also sound as of chimps moving through grass. After 6:30 all was quiet.

3. NOMADISM, LOCOMOTION, ACTIVITY DISTRIBUTION

General migratory movements on the part of chimpanzee groups have not been recorded (9, p. 248). My observations, while showing that the group wanders about considerably during the day, lead to no conclusions regarding extensive movements persisting in the same general direction for longer periods. In this connection it is significant to note that chimpanzees are reported to inhabit the region in which I was working throughout the year, regardless of season. This report receives some substantiation from the fact that Professor Yerkes, at the time of his visit to Pastoria in 1929, had opportunity to observe a band of the animals not far from the scene of my later operations. That was in the latter half of the rainy season. The fact that chimpanzees are always found in a certain district does not mean that they never wander great distances; it is entirely possible (but not very probable, I think) that a band sometimes migrates hundreds of miles. That possibility seems to necessitate the supposition that the causes for such migrations are "random" and are unrelated to definite environmental factors like climate or

food and water supplies. Since such environmental factors presumably affect all groups alike, a constant population in a given section would be inconceivable.

What is certain, however, is that the chimpanzee is nomadic, having no permanent home, wandering about during the day as his appetites and whims (or, if one prefers, external and internal stimuli) lead him, and setting up camp wherever nightfall overtakes him. Sometimes, indeed, it happens that a group will end up at twilight very near where it started in the morning, having wandered at least a mile or two away in the meantime. I have never seen a group stay within an area of, say, one mile diameter, the entire day. It is difficult to define the specific influences which directed these wanderings. Perhaps remembered or directly perceived food and water sources, or good resting and play places, or the presence of enemies to be avoided, were determining factors. I suggest that among other possible motivating conditions may be the need or urge for exercise and for changing and novel environmental stimuli.

In the case of many groups which we had under observation, the stimulus for movement was probably discovery of our presence. Food sources certainly had something to do with the direction taken by these wanderings. Certain hillsides or highlands where the Naray grows in abundance were relatively deserted except during the time when this fruit was "in season." Water was usually so ubiquitous that wanderings could not well be correlated with its presence or absence.

Let us accompany one or two groups of chimpanzees on their wanderings. As we go along, we shall incidentally gather data on certain habits or types of behavior which are not given special treatment in later sections, and shall also learn something of the sequence and temporal distribution of various activities.

April 9. Started at 4:30 for marigot B. Travelled along west side, northward. At 5:30 (half-hour after sun-rise) heard chimp cries from our northeast—seemed to be coming from the valley. Pursued sounds which soon stopped. Searched unsuccessfully for almost an hour. Then heard cries again, to our south, which we followed. Cries became more frequent; included very often the F-P cry, the low bark and the ex-

citement, high-pitched cry.   Also, very loud and voluminous, TT sounds.[2]
Animals were apparently still travelling, for sounds kept preceding us.
Finally we seemed to be fairly close, so I left the porters and proceeded
alone, taking Busch and Leica.   The marigot valley here is very flat,
wide and extremely thickly wooded.   Progress difficult; often had to
crawl in prone position through underbrush.   Marigot from a few inches
to about three feet deep—average width, 20 feet.   Flows south to north.
Chimps apparently still going south.   Was able to follow chimp trail
occasionally; along it I found, 1) a very new fecal deposit, greenish in
color, firm, and containing small quantity Naray peas; 2) several places
where a fairly large area of grass and bushes had been trampled down—
evidently rest, play or fighting places of the chimps.   During my pro-
gress the animals could be heard at approximately 15-minute intervals,
and always the F-P cry was prominent.   Was bitten by ants.   I finally
tired of the difficult progress through the underbrush and emerged on
the east side of the valley, this being a high, very steep and fairly bare
hillside.   Proceeding southward, about one-third of the way up the
incline, I suddenly saw (9:35 A.M.) a large number of chimpanzees in a
Gerenyi tree, about 500 feet to the south.   The tree overhung the mari-
got, but was rooted on the east bank.   Certain there were at least 10
animals; possible that there were two or three more.   Saw no infants.
Watched the animals feeding for some 15 minutes; then by 1's and 2's
and 3's they gradually descended.   Do not believe they saw me, for I
was well hidden by a rock and bushes.   Soon I heard chimp cries from
further up the valley, to my south.   Feeling quite certain that all the
animals had left tree where I had seen them feeding, I descended cau-
tiously to the marigot and there took up the chimp trail.   (While in
the tree feeding, the animals had made no loud sounds, but soon after
their descent cries and TT sounds started in again.)   The trail crossed
the marigot twice, but in each case it was at a place where the animals
did not have to get their feet wet; in one instance there were sufficient
rocks and patches of solid ground, and in the other case there was a fallen
tree which formed a bridge.   When about 75 feet from the Gerenyi tree
where the animals had been eating, I suddenly and unexpectedly came
into full view of that tree; an adult chimpanzee, making no sounds, but
giving me several quick looks, was rapidly descending and then quickly
and noiselessly disappeared into the heavy undergrowth to the south.

[2] The abbreviations, F-P cry, for fear-pain cry, and TT sounds, for tom-tom
sounds, are used in all of my field notes.   Both terms are more fully explained
in our Section 7.

The thing happened too quickly and unexpectedly for me to get a picture of the animal. It was 10:15 A.M. Under the tree I found broken-off branches, some still bearing fruit. Progress from now on, I saw, would be even more difficult than it had been; also I was quite a distance from my porters and supplies. Turned back at 10:40, and after getting lost for an hour or so, located my natives. Ate and rested. . . . . Started out again about 2 P.M., going south, but this time along west side of marigot. Heard a few cries to our northeast, and searched in that direction; found nothing. Again went to west side of marigot B, and then southward. At 4:45 heard loud chimp cries from the south. We stopped on an elevation some 200 feet west of the marigot; it commanded a good view and yet we were well sheltered by bushes. This was about $\frac{1}{4}$ mile north of where I had seen the animals eating in a Gerenyi tree that morning. At 5:15 a volley of chimp cries from E, SE, not far. Five minutes later saw an adult chimp standing on a large rock at the top of the steep incline on the east side of the marigot. He stood there tranquilly, on all fours, for about two minutes, apparently surveying the surrounding country. Then he got down from the rock and sat down next to it for another two minutes, after which he walked away slowly and disappeared into the woods to the east. I could see, very dimly, several other chimpanzees moving in those woods. Was under the impression that the animals were there to make nests, on the top of the hill. Then, coming from the marigot jungle below us, three more good-sized chimps ascended the slope; they were in plain view, since there was little vegetation there. They moved slowly and at a slight angle to directly upwards. Then a large female, genital area swollen and red, ascended, a very small chimp walking by her side. Part of the way the large animal put one arm around the smaller one, but I do not think that she ever really carried it. As mother and child were ascending, two adult animals descended the slope; the two pairs passed each other at a distance of 10 or 15 feet, but apparently neither paid any attention to the other. All animals moved slowly, resting now and then. One of the two descending animals used the "swing walk" part of the way. The incline is very steep—probably 20 degrees or more; dirt, gravel, rocks, and, at this point, few bushes and no trees. The two descending animals hesitated and stopped often to look around; they disappeared behind the heavy vegetation near the marigot. Shortly thereafter ten animals, none of them very small, ascended the hill in same leisurely fashion; disappeared in woods on top. Chimp cries still continued to come from the marigot jungle; there were no sounds

from the top of the hill. I thought that most of the animals had now gone into the highland forest to make nests, and that the few stragglers, still below, would soon follow. But I was wrong. Starting at 5:40, nine chimpanzees, including no infants or very young animals, appeared at the top of the slope, in single file, and started descending. The first animal in the file was the largest. Light was getting dim and distance was too great to distinguish sex. Appearing one quite close behind the other at the top of the hill, they spread out in all directions during the slow, protracted, hesitant descent. Some used the swing walk exclusively, some used it part of the time, and others did not use it at all. Here and there they stopped, usually next to a low bush (really not bushes at all, just shoots of green stuff a foot or so high), eating listlessly the leaves thereof. For ten minutes or so they sat quietly. One large animal seemed to sense that something was wrong; he looked directly at us several times. Hardly possible that he could have seen us; we were perfectly quiet, were well hidden, and were at such a distance that all my observations were with the binoculars. Gradually most of the other animals looked in our direction also, but were not as persistent about it as was the first, large chimp. At about 5:50 the animals resumed the descent, disappearing into the marigot jungle singly or by 2's and 3's. All of them disappeared from view at the same point. One animal, obviously an adult, remained for some time after the others had gone; he had a small branch of Naray pods. Would eat contents of one pod, go a little way, sit down, eat some more, and so on. As this animal was disappearing, it was still carrying the branch in its mouth. At about 6:10 P.M. the first animal ascended a Gerenyi tree growing right next to the marigot, but a little isolated from surrounding vegetation; the tree was in plain view. (The whole thing was running itself off like a stage performance; we had gallery seats.) Other chimps soon followed the first one up into the tree, and nest building began. Could plainly see one animal at work; its movements were rapid but unhurried. There was no hesitation or interruption; the animal worked as if it knew exactly what was to be done. The building of that nest took just three minutes. Could see no infants in this tree. There appeared to be 11 or 12 animals. Breaking of branches made loud sounds. Cries, not nearly as loud as earlier in the day, including the F-P cry, continued until 6:35. Some of the animals protracted their nest building in order to eat of the Gerenyi fruit. Sounds of nest building continued until 6:50. These sounds could also be heard coming from the place where I had heard cries while most of the animals were on top of the hill, but I could see nothing of this

place—it was too low and there were too many trees in the way. At 7 P.M. there were several choruses of panting cries—started by one animal and joined in by many others. Not loud, rather pleasing harmony. A good-night song? I had sent porters back to Kambi for something to eat and for various supplies. Spent the entire night on the spot, sleeplessly, with Moma. The moon, well over half full, gave enough light to read by at 11 P.M. It set at 2:45 A.M. Breeze was blowing most of the night; it seemed cold. Frogs gave concert part of the night. A few birds and bats flitting about. A record of chimp cries after 7 P.M. follows; at no time was there sound or sight of chimps moving around:

7:15—Short, low panting cry, one animal.

7:30—I lit a match. Short panting cry, two animals.

7:35—Very short, low, soft cry, one animal.

7:45—Very short, low, soft cry (tinged with fear?), one animal.

8:00—Warning call of the green monkey, nearby, followed by a short chimp cry, not loud, one animal.

8:05—Repetition of green monkey warning call, but no chimp sounds following.

8:30—Warning call of green monkey—then a short panting cry by one chimp.

8:50—Green monkey warning call again—no chimp sounds.

9:15—Panting cry, about 20 sec. duration, louder than those before, one animal.

9:35—Very short, low cry, one animal.

9:45—Very short, low cry, one animal.

No more cries until after 5 A.M.

At 5 A.M. it began to get light. As soon as the eastern horizon showed the first bit of light, there were signs of chimpanzee life both in the nesting tree visible to me and from the spot some 250 feet north of there not visible to me: short panting cries and moving about in the trees. Could see animals eating in the Gerenyi tree shortly after that. By 5:15 some of the animals had descended to the ground, and from then until about 6 A.M. could be heard, at irregular intervals, F-P and anger cries. A little after 6 most of the animals went to the ground, two only remaining until 6:45. Sounds of animals moving about on the ground continued until 7:10. After 7:30 chimp cries were heard coming from perhaps 1000 feet further south. My scouts reported that the animals who had nested where I could not see them, disappeared in a northeasterly direction this morning. Quite clearly, these animals formed a separate group which had come from the northeast the preced-

ing afternoon and returned in that direction this morning; apparently
the two groups, although nesting a few hundred feet from each other, had
not mingled or had other close contact.   Green monkeys were seen in a
tree 50 feet from the observed nesting tree, at 7:15 A.M.   Under the
latter I found three urine deposits, one colorless, two dark brown.   Also
found several fecal masses, a few containing stones and completely
undigested Gerenyi fruit, and one containing many "peas" of the Naray.
The reason I did not find more excrements is probably that the ground
here is covered with leaves and undergrowth.   Ten nests were found in
the tree, all apparently very new; they were from 60 to 85 feet above the
ground.   (Nests of the other group could not be found, despite diligent
searching.   This is not overly surprising, since the dense vegetation
hid the upper parts of many trees entirely from view.)   Followed the
chimp trail southward some $\frac{1}{2}$ mile; it crossed the marigot twice, each
time at a place where the animals did not have to get their feet wet.
We probably could have observed the group further that day, since their
cries were still audible to the south at 8:45.   But I was too tired to con-
tinue work, and we returned to Kambi.

The wild chimpanzee is an early riser.   This fact is illustrated
in the above passage and found verification in all of the twenty-
odd instances when I had a group of animals under observation
at break of day.   I was told by natives that during the rainy
season the chimpanzee often delays getting up until an hour or
more after sunrise, but certainly during the dry season he is up
and around before the sun has risen above the horizon.   Some-
times, in fact, I heard these animals moving around while it was
still completely dark.   They do not build nests, in the dry season,
until advanced dusk and lie down to sleep only after it is almost
completely dark.   On one occasion, in fact, I heard them making
their nests an hour or so after the sun had set.   The apparent
difference in sleeping habits of chimpanzee in dry and wet seasons
is to be explained, I believe, by the obvious aversion of these apes
to exposure to direct sunlight.   During the season of rains the
sun remains hidden behind a blanket of clouds all day long, and
the animals can eat and travel at leisure.   But in the cloudless
dry season, feeding in trees and travelling over sparsely wooded
areas must be done early in the morning and late afternoon if the
burning mid-day sun is to be avoided.   Rarely did I see chim-

panzees feeding in trees, which were not shaded by higher trees, between 10 A.M. and 5 P.M. and never did I see them travelling over open country during that part of the day.[3] The principal feeding periods, then, are in the early morning and late afternoon, although some feeding occurs at irregular intervals throughout the day (see also pages 65–66). Most of the travelling is also done before 11 A.M. and after 4 P.M. Whatever travelling the animals may do while the sun is high is in deeply shaded forests or through the ribbons of jungle-land which follow the courses of streams.

The larger animals, especially, spend a good part of the heat of the day in resting and, perhaps, in sleeping. Such periods of quiescence may be spent on branches or crotches of shady trees or, more often, on the ground. I never saw anything to indicate that chimpanzees occupy tree nests, in which they sleep at night, during the day. When their daytime rest is taken in trees, the animals usually assume a reclining position, either on the side or on the back. A foot or hand usually grasps a nearby—generally overhead—branch, probably to give greater security. Sometimes, however, an individual squats on a branch, using the trunk of the tree as a back rest. When the animals rested on the ground they always picked a shady spot, usually in a valley and often where approach was difficult because of heavy underbrush. Sometimes they sat or reclined on whatever happened to cover the ground and sometimes they bent over bush grass, pulled out small bushes and very small trees or broke off leafy branches from nearby trees and lay down on these. These latter improvisations I have termed "day-beds", they have nothing in common with the nests used at night except that both are used as beds in the broadest sense of the word. Occasionally these "day-beds"

[3] In this connection it is interesting to note that whenever I placed the box, in which I kept a captured infant female chimpanzee, into the direct rays of the mid-day sun, she screamed continuously and appeared very uncomfortable. Once, after a ten-minute exposure of this kind, the animal showed a body temperature of 102. Although the exertion of screaming may have contributed to this rise, it would appear that the temperature-regulating mechanism is not developed at an early age in these apes.

had roofs or sun-umbrellas covering them, formed by bending or twisting young saplings so that their leafy tops shaded the day-bed beneath.   I saw six such canopied day-beds, and all of these were near the margins between jungle land and open fields. Usually the resting places of chimpanzees were in places which required no artificial shading.   On April 12th I had a group of about ten animals under observation early in the morning.   There had been a great deal of drumming and vocalization around 7 A.M.

April 12.   By 7:30 things were quieted down.   I ascended the south side of the shallow valley.   Could hear two animals breathing hard: —ha—ha—ha, as dogs do in play-fighting.   One animal, medium sized, came towards me, but turned back when about 25 feet away.   This happened a second time, 25 minutes later.   In the meantime the sound of breaking branches and the cracking of dry bush grass could be heard. One animal seen lying on the ground.   I attempted closer approach, crawling.   All sounds ceased suddenly—the animals disappeared. Where they had been, I found: 1) Two areas, about 25 feet square each, where the bush grass was trampled down flat.   This was in the open field which skirts the marigot jungle.   2) Five places, right on the out-skirts of the jungle where leafy branches from neighboring trees were on the ground; it was clear that they had been lain upon for some time. Three of these places were naturally shaded, two of them not so.   In the latter two cases a small sapling had been bent over so as to form an umbrella exactly over the spot where the leafy branch was on the ground. Very comfortable for an early siesta. Call these day-beds. . . . .

The literature contains no definite indication that the wild chimpanzee sleeps during the day, and the belief has been ex-pressed (9, p. 228) that they do not do so.   Some indirect obser-vations lead me to suspect that there may be some exceptions to this rule.   On two different occasions I was close to a group of animals who were apparently resting and playing on the ground in a deeply shaded area.   The thick underbrush and conformity of the ground allowed me only glimpses of the animals, but I could hear plainly the steady, slow, and somewhat noisy breathing characteristic of sleep, of two or three animals.   On another

occasion (April 13), I had this experience: A group of animals had been traced to a thickly wooded, shallow valley. I left my porters behind and proceeded as quietly as possible. About 100 feet ahead I saw two animals walking slowly on the ground. Advancing more slowly than ever, I got to within 50 feet of where I had seen the two chimpanzees. Suddenly I was startled by a loud bark not more than twelve feet to one side of me, and saw a large chimpanzee running away as fast as he could go. At the time I had the firm impression that I had awakened the animal and that his outcry was decidedly a reflex sort of response. Certainly it was the first and only time that I heard a chimpanzee give vocal utterance when danger threatened. And it is not probable that the animal was awake and yet did not hear or see me coming until I was 12 feet away. The only other times I got that close to a chimpanzee in daytime were when the animals approached me and I was lying quietly in a well-sheltered spot.

My notes for April 9 and 10, quoted above, indicate that the chimpanzee uses his nights primarily for sleep. Many of the outcries recorded are correlated with obvious disturbances, such as my lighting a match, and the warning cries of green monkeys which probably signified the presence of other animals, perhaps wild-cats, panthers or serpents, nearby. One other night (April 5) I spent near a group of sleeping apes. On this occasion the moon set about 11 P.M. and, until I fell asleep at 1 A.M., there were no cries later than one hour after sunset. Once, as we were coming through the bush at about 10 P.M., there was an uproar of chimpanzee cries, one-half mile or so distant. This vocalization was provoked, I suspect, by the flashing acetylene head-light which one of my porters was carrying. Only twice, in all of the many times I was in the field an hour or more before dawn, did I hear chimpanzee cries earlier than 15 minutes before daybreak. Natives told me that on moonlight nights these apes sometimes vocalize all night long without leaving their nests. They speak of these as carnival nights. Although I have no right to be skeptical, I suspect that such sleeplessness may have its cause in disturbing factors, such as the presence of enemies, or may possibly exist only in the imaginations of my informants. One

incident, reported in another connection (see page 48), shows that the chimpanzee will forsake his nest during the night and descend to the ground under extreme provocation, even when there is no moonlight.

Reference was made, above, to the "swing walk" which chimpanzees used in ascending and descending an incline. This mode of locomotion appeared to be rather exceptional, being observed only twice. Yerkes and Yerkes (9, p. 215) describe it as follows: "Frequently observed, also, is use of the arms crutch fashion, so that the body is swung forward with weight wholly on the arms until the legs are well forward between them." More usually observed was a rather slow, straightforward walk, and various forms of running and leaping. Both in walking and in running, the main body axis of the animal often formed an appreciable angle with the line of progression. I am convinced that the chimpanzee can move faster over the uneven, underbrush-covered ground of his native habitat than can man, but doubt that his speed under these conditions would exceed that of a fast human runner on a smooth track. The chimpanzee is usually leisurely in locomotion; I saw him running fast only when startled by our unexpected proximity. Climbing up the trunks of trees, even those measuring four or five feet in diameter, apparently causes the ape no more trouble than travelling on the horizontal plane. This is especially true of large individuals. The animal seems just to walk right up. Climbing down a tree seems to occasion more difficulty and require more time than its ascension. In coming down a tree, the animal goes backwards, if the trunk is very thick, but if the trunk is of small diameter he sometimes descends head first. A much more common way of getting down from a tree which has branches fairly close to the ground is as follows: The animal walks out on a low branch which bends under his weight until he can either step off onto the ground or, by hanging from his arms, make a short drop to the ground. The chimpanzee rarely travels very far in a horizontal direction in trees. If he is going somewhere he usually first descends to the ground. This is in marked contrast to arboreal monkeys of the vicinity, who are seldom seen on the ground, and who travel with

unbelievable rapidity through the tree tops. Occasionally, how-
ever, the chimpanzee also goes short distances by the tree route
and this sometimes necessitates jumping from the branches of one
tree to those of another. Such jumps do not carry the animal very
far horizontally, but appear always to carry it downwards; the
greatest leap I witnessed must have involved a drop of at least
40 feet. Landing is, of course, softened by the great elasticity
of branches which bend deeply as the weight of an animal strikes
them. In all jumps of this kind which I observed, the take-off
was the same: the ape first hung by his arms, swung back and
forth, and let go on a forward swing. In general, the chimpanzee
seems equally at home on the ground and in trees. Comparing
him to other primates of the region, we would say that he is far
more arboreal than the baboon and very much less so than the
other monkeys. I should guess that he spends about one-third
of his waking hours in trees.

No evidence was obtained that the chimpanzee ever goes
swimming, and a number of observations indicate that this ape
is rather water-shy. This is in accordance with native tradition
and current scientific belief (9, p. 215). Whenever a chimpanzee
trail crossed one of the numerous small streams of the region,
it was always at a place where the animals did not have to get
their feet wet; sometimes it was at a place where the water flowed
underground for a short stretch, or where boulders and patches
of ground provided a dry passage, and sometimes the crossing
was effected on overhanging trees or over a fallen tree-trunk
which formed a natural bridge. Seven or eight places were found
where chimpanzees had evidently stopped for some time at the
water's edge; grass and ferns were trampled down, nearby
branches had been broken off, and feces were on the ground.
Only once did I observe animals at such a spot, and then only
briefly. On that occasion, only one animal, about six years old,
paid any attention to the water; he repeatedly rushed to the edge
of the stream (2 or 3 inches deep, here), stopped short, "spanked"
the water with one hand, and then bounded away again. My
helper, Moussa, watching from a slightly different position, said
that he saw an animal gazing intently down into the sluggish

water, and then tossing his head and grimacing. Perhaps the ape saw his reflection in the quiet water.

We can, perhaps, best summarize this chapter, and at the same time supplement our picture of a day in the life of free-living chimpanzees, by quoting fully from our notes of April 21.

April 21. Started at 5 A.M. for marigot valley C where we had heard chimpanzees building nests the night before. As we arrived, could hear animals in trees; also there were two short F-P cries. Some of the animals were evidently feeding on Tumbeh-Naray, while others were on the ground. By 5:55 they had left. Could find only four nests; there are many trees here, the smaller ones obstructing view of the higher ones. . . . . The animals had apparently departed westward, into the thicket north of Kambi. We ascended the little hill to the north, and at 6:10 saw 5 animals feeding in 3 different Naray trees on the long slope to our northwest. Watched them with the binoculars for some 10 minutes, after which they gradually descended. We approached. At 6:40 heard cries further north. Followed the sounds which recurred about every 5 or 10 minutes; mostly the F-P cry. The animals were evidently travelling northward, for we did not seem to be getting any closer. At 7:30 we suddenly saw chimpanzees, in a large, high Kanti tree, eating. We were some distance away but did not dare approach closer, since the intervening ground was rather bare. There were certainly 6 and probably 9 or 10 animals in the tree. They stayed some 6 or 7 minutes, making considerable noise climbing around in the branches; one large branch was broken off, we later found it on the ground. Then they descended. Under the tree we found 8 peelings of the fruit; the job was neatly done for the entire skin was still in one piece, though divided into sectors such as one gets in peeling an orange with a knife. No stones of Kanti were found on the ground. From this point we followed the trail in traditional hunter fashion. The ground here is sandy in many spots and many chimpanzee footprints could be found, some two or three times as big as others. On the trail we found Wehyenyi—mostly pieces of its skin or hull, but also some skinned, uneaten pulp. This fruit was growing near the trail. Also found one fresh, formed stool, pasty, tannish green, containing three Kanti stones and several stones the size and shape of Brazil nuts (?). . . . . The trail led to a shallow but heavily wooded ravine which opens into the valley of the Susidé marigot. 8:10. I entered alone, very cautiously. Could hear two animals making a commotion on the ground; heard them breathing

hard and "snarling". This stopped and I heard an animal coming to-
wards me. It was a medium-sized animal, light-colored face. Suddenly
he saw me. He stopped short and stood upright, looking towards me.
Then he ran a little distance and climbed up on some woody vines, again
looking at me. (Incidentally, I never saw a chimpanzee in the bush
walking upright, although on several occasions I saw them standing on
their two hind limbs.) Then he descended and quickly disappeared. A
moment later I thought that I heard a dull thud—ground-slapping? I
followed. Came to a clear space which I crossed. Just as I was about
to enter the thicket again, there was a very sudden noise as of 50 chim-
panzees galloping away. I did not see the animals, but am sure there
must have been 10 or 12, at least. I was probably about 50 feet from
them when they rushed away. The first time that I observed concerted
action like this on the part of chimpanzees; the action was as if in re-
sponse to a military command. No vocalization. . . . . I went back
to my porters, and we resumed march northward. At 8:50 chimp cries
still further northward. We followed, crossing the la Kolenté
road. . . . . Came to the long slope which leads down to the RR tracks.
10 A.M. From there I could see several animals (3 or 4) moving about
in trees. With the binoculars I saw a large animal on the ground, eating
some small fruit. She seemed to be spitting something out into her
hand and then put her hand in front of her, near the ground. On the
ground, between her legs, was an infant. After some 8 minutes of obser-
vation she departed, the infant clinging to her ventral side. The other
animals also departed soon, and a little later we could hear cries from
further down the slope. We went to where the chimpanzees had just
been. Found a large formed stool, light green in color, pasty, containing
9 stones of Kanti but no other undigested material. Also found a mass,
about the size of my fist, of the inner part of Bomenti; a few bits of the
shell were also in the lump of stuff. My natives immediately had an
explanation: The mother chimpanzee had shelled a quantity of the
Bomenti fruit and had then pressed out the juice with her hand into the
open mouth of her infant. Moussa claims that he has seen this often.
In view of my earlier observations, the explanation appears very plaus-
ible. Could not find any Bomenti growing in the immediate vicinity.
At 10:20 we heard chimp cries coming from the bottom of the slope; the
animals were apparently at the marigot. The F-P cry predominated—
it seemed to come from one or more small animals. We descended the
rest of the way and rested near the marigot. The vegetation is very
thick here. At 12:15 there were loud cries, perhaps 400 feet west of

where I was resting. (A train passed at 1:20; less than 500 feet from where the animals were.) These cries were repeated at frequent intervals, with the short (anger?) barking prominent. At 1:10 and 1:15 there were short solos of the F-P cry. At 1:20 short solos of abbreviated panting cries. At 1:30 we went to the RR tracks and walked along them to the west. Could see violent and repeated disturbances of the trees on the other (south) side of the marigot. Occasionally we got glimpses of a black form moving in those trees. Cries were not so frequent now. After about 10 minutes of observation, saw one medium-sized chimp walking on all fours on the ground, going east. I went southward from the tracks alone, until close to the marigot. After my eyes got accustomed to heavy shade I could see two animals rushing around in a couple of trees; evidently they were playing tag. No vocalizations. To my left (east), however, could be heard, frequently, a high-pitched whimpering sound of short duration; not loud. The animals playing tag soon descended. TT sounds, accompanied by cries, very loud, occurred once, to my south. I went further south and crossed the marigot. A large female was lying on the ground, back towards me, about 50 feet south. Sleeping? Near her head was a second animal, white-face, sitting or half reclining on the ground. This second animal shifted its posture several times; part of the time was playing (?) with something on the ground. I was in well sheltered place and remained perfectly quiet. Other animals could be heard nearby walking on the ground. No vocalization at this time. After about 20 minutes I suddenly saw, to my right, perhaps 100 feet away, two animals evidently copulating. Saw almost nothing of the female; evidently dorso-ventral position. The large male gave about six fairly slow thrusts; immediately thereafter disappeared from view, although movements of foliage indicated their continued presence for some five minutes more. In the meantime the first two animals had left. Had they seen me as I tried to get better view of the reproductive behavior? 3:20 P.M. . . . . Vegetation thick, hard travelling. Could not find the animals. Vocalization had ceased an hour earlier. Returned to my porters at RR tracks who reported that animals had moved eastward. 4 P.M. Proceeded in that direction and soon came to a place where we could see the chimpanzees moving on the ground, southward (up the slope). They were in single file, as usual, following each other at short intervals. I counted 8, my porters claimed they saw 10 or 12. . . . . We followed, but saw and heard no more of the animals that day. . . . . April 22. . . . . Sugeta reports that a large group of chimps was seen

and heard last night and this morning near a little village about one-half mile east of where we had last seen the animals yesterday.

4. NESTS AND NESTING

Products of wild animal activity usually are more easily observable in the bush than is the activity itself.   This is pre-eminently true of chimpanzee nests and nest-building behavior. It would be difficult to walk far through the territory in which I was working without seeing chimpanzee nests.   But the casual stroller would be fortunate indeed to witness the process of building nests.   In spite of serious and persistent effort, I had only two really good observations of the activity.   There are several reasons for the difficulty.   The animals never, in my experience, started nest-building before dusk; the possibility of long-range observation by means of binoculars, therefore, was tremendously reduced.   Often nest construction was delayed until it was so dark that one could see practically nothing, even if in a favorable position.

April 6.   We walked E along the dry stream-bed.   It was quite dark except for light afforded by a half-moon.   6:25 P.M.   We left the valley and then skirting the marigot jungle on the N (going E), suddenly heard, in the forest, the breaking of branches.   Evidently chimps were just constructing their nests.   At about same time that we heard breaking of branches, there was excitement or panting cry—not very loud—coming from several separated spots in the forest.   This is the first time I felt that chimps were making sounds when danger threatened.   It sounded like communication of common unrest.   Nothing could be seen of the animals, of course.   We passed along in order not to scare them away from the spot, and sounds (cries) promptly ceased.

Secondly, the animals seemed to be more alert to signs of danger just before building their nests than at any other time of day. Lastly, I found no means of predicting where a group would nest, and so it was not possible to prepare and get into a blind beforehand.

Chimpanzee nests were found in the trees of dense valley forests and in those of the more sparsely wooded hills and highlands as

well.   In the former case, nests are often most difficult to find, dense foliage effectively hiding them from view.   On the highlands, to the contrary, they may be easily visible, sometimes being located in trees standing at the edge of a grass field.   It would not be profitable to attempt an estimate of the relative number of nests found in valleys and on the highlands, respectively, especially since so much territory cannot be classed definitely as one or the other.   In general, I should say that I saw more nests in thinly wooded areas, but this may only mean that they are more easily visible there.

Nests were built in about every kind of tree, growing in the bush, which bore leaves.   (Certain trees, notably the Fromager, lost most of their foliage towards the end of the dry season.) It seemed very much as if the selection of a given tree as a nesting place was largely a matter of chance.   Many of the trees containing nests bore edible fruits; as many others had nothing of food value to offer the animals.

April 4.   A short distance down from the top of the hillside, there were four trees containing nests (apparently *very* new).   One Naray tree and two non-food trees were closely adjacent, while a second Naray tree was about 175 feet away.   In the group of three trees the Naray had 5 nests, 30, 32, 35, 35 and 45 feet, respectively, from the ground; one non-food tree had four nests (23, 27, 30 and 48 feet), and the other one had only one nest, medium sized, 24 feet high.   The distant Naray contained 3 nests, 30, 35 and 38 feet above ground level.

The foliage of nesting trees varied markedly; some had large, wide leaves, while others had needle-like leaves resembling those of the fir or spruce.   Palms were never utilized for nesting, as far as I could determine.

The chimpanzee nest is a distinct and unique feature of the French Guinea bush.   It is so different from all other objects encountered there, that it is almost impossible not to recognize such a nest, when seen, for what it is.   In view of this fact, we may say without further qualification that chimpanzee nests, at the time and place of our observations, were always built in trees, never on the ground or in bushes.   An account of daytime

"beds" and shelters, which bear no resemblance to true nests, is given above (pp. 31–32). In conversation with French-speaking natives I found that they always referred to the "case" of chimpanzees and apparently did not know what I was talking about when I spoke of the "nid." Further inquiry brought out the fact that the native language has only one word for both "case" and "nid," a word which seems to have the general meaning of "shelter." This discovery leads to the suggestion that early reports on the subject, to the effect that chimpanzees dwell in huts or rude houses, may have their explanation in the language limitations of native informants.

I found from one to thirteen nests in a single tree. In the case of the larger figure, I was not able to determine whether all of these nests had been constructed and used on one occasion or whether some of them were a day or two older than the rest. The largest number of nests in a single tree, all of which, I feel sure, were used on the same night, was ten. Often there was only one nest in a tree, and this might be large, small, or of average size. Although all members of a group did not, usually, sleep in one tree, their nests were rarely scattered over an area more than 200 feet in diameter.[4] Occasionally, one nest or two would be found in relative isolation, half a mile or more from the nearest discoverable nests of approximately equal age. Such cases formed conspicuous exceptions; in the majority of cases discovery of one nest was followed by the finding of others nearby. My scouts reported that one evening they saw ten members of a group building nests close together, while the eleventh member, a large male, made his nest in a tree some 700 feet away. Observation of the animals the following morning, and examination of the nests, made the story appear very plausible.

Using a Leitz range finder, I made 100 measurements of the distances of nests from the ground. The figures vary from 13 to 105 feet, the average being 38.4 feet. Only six nests were 20 feet or less from the ground, and seven were 75 feet or more above ground level. In general, the nests found in valleys were

[4] This statement must be accepted in the light of our uncertainty as to what constitutes a group. See p. 22.

further from the ground than those of the highlands. This is doubtlessly due, in part, to the fact that the average height of trees in the valleys is greater than that of trees growing where water is less plentiful. Except that the three lowest nests found were small, no correspondence was discovered between the size of nests and their relative elevation in the tree. Large nests involve the use of thicker branches than do small ones, but the thickness of branches varies with horizontal distance from the center of the tree as well as with vertical distance from the ground.

The structure of eight nests was examined in detail. The following extracts from my original notes pertain to what we may call "typical" nests.

March 5. The 9 nests were in a Naray tree, about 50 feet high. The nests were from 25 to 40 feet above ground, the highest being near the top of the tree. Noticed that the biggest nests were on the strongest branches. This is natural, since smaller animals not alone require less supporting strength, but probably can break only weaker limbs of the tree. . . . . One nest was examined in detail and taken apart. It was almost flat; that is, it had practically no sides. Twigs of the piny needles or leaves were found loose on the top layer of the nest. No evidence of stripped leaves. It may be that part of the smoothness of the nest is given by the pressure exerted by the chimp sleeping on it a whole night. That is, when the animal first lies down, the nest may not be as smooth as we find it the next day. The nest had a definitely circular—slightly oval—pattern. Most of the branches composing it were bent backwards, i.e. from peripheral end inwards. Several pod hulls, stripped of their contents, as well as small particles of the pods, were found in the nest. Otherwise it was perfectly clean. . . . . The nest showed definite and certain evidences of intertwining, especially of the larger branches which bore the chief load. This intertwining is necessary in order to produce the required stresses and to give desired shape to the nest. The nest investigated was the largest or second largest of the group; its diameters (horizontal) were 20 and 16 inches, respectively, outside measurement.

February 15. Found the nests which the group had occupied the previous night, 9 in all. Some large, some quite small. Seemed to be constructed solely of the materials of the tree in which they were built,

and since the 9 were in four different kinds of trees, the nests presented very different pictures. . . . . The first examined was in a Naray tree, being almost at the top. . . . . The leaves of this tree grow much like pine-needles, and look like them. Bright green in color, flat, rather sharp points. The nest was well padded with these needles; on the top surface practically no wood was visible. It was egg-shaped; outside diameters 23 and 15 inches, inside depth 7 inches. Symmetrical. Very springy and doubtless a comfortable resting place. The underpinnings were branches from all sides which had been broken and bent backwards, sidewards, downwards and upwards. Some of these broken branches were a good 2 inches thick. The nest as a whole rested on a horizontal fork of two main branches of the tree. . . . . At 4:30 I examined the second nest. Leaves of the tree were green, large, almost round. . . . . The tree bears no fruit or flowers at this time. Nest was built only of materials of that tree. Very smooth inside; lined completely and thickly with leaves. As in previous case, most of the leaves lining the upper surface of the nest belonged to the broken branches which formed the foundation of the structure. In other words, the branches were broken in such a way that their leafy tips just reached to the floor of the nest. In addition, there were a few leafy twigs from other parts of the tree which increased the padding of leaves. The nest was about 2 feet above a vertical fork of two main branches, but it did not rest in or on the fork. Again, the support was formed by branches which had been bent or broken from all directions towards the locus of the nest. One branch had been broken 6½ feet below and to one side; it formed an important element in the support. It seems to me that the principal secret in successful construction of chimpanzee nests lies in the breaking of branches, breaking the right ones at the right place and at the right angle. The nest was resting mainly on three branches, each of which had been broken at some distance. These branches were "green," so that although perhaps one-half of the fibers were severed, the rest remained intact. There was some evidence of intertwining of the secondary branches and twigs to lend added solidity and cohesiveness to the structure. Inside diameters 17 and 12 inches, greatest inside depth 2 inches. Commanded a good view of the surrounding territory.

April 4. One nest I examined in detail. It was very strongly made, resting in a two-way crotch. Many large and small branches bent forwards and backwards in its construction. Some of these branches were partly broken, while others had enough elasticity not to break. On the

top of this nest, definitely concave, was a large leafy branch from a neighboring non-food tree. This branch could not have been reached from where the nest was located, but it is conceivable that the chimpanzee jumped from the non-food tree, with a branch, to the tree containing the nest. Underneath the non-food branch, there were many loose twigs from the nesting tree itself. Most of the padding, however, came from the leafy twigs of the branches which had been bent to form the framework. There was no evidence of twigs having been stripped of leaves. Under the tree was a large branch which had been broken off very close to where the examined nest was built. Perhaps this branch had been intended as part of the structure but had not stood the strain. Another possibility is that the animal wanted a clear view of the sky above (or for some other reason did not want a branch directly over it), and had therefore simply removed the branch as being undesirable. One thinks of the possibility of a snake or panther attacking from a branch hanging low over the nest. It is true, I think, that most, but not all, nests are so placed that the space directly above is clear. One branch, forming an element in the structure, had been partly broken 5 feet from the edge. Inside depth 5 inches, outside depth 10 inches. Outside diameters at top, 18 and 16 inches. In this group of nests, the largest ones were at mid-distances, while the smallest were highest and lowest in the tree.

The great majority of nests varied only in minor details from the above descriptions. Notes on some atypical features follow.

April 3. Near the junction of the long slope facing south and the ravine which cuts into the plateau, we found 7 nests of "the day before yesterday" (statement of guides). They were scattered among 5 rather low, non-food trees; in these same trees there were 4 obviously older nests. One of the new nests was very crude and skimpy; it consisted merely of three small branches bent in a circle on top of a 3-way crotch. This was the only nest in a scrawny tree; the amount of branches, twigs and foliage did not permit the construction of a more elaborate nest. It was 26 feet from the ground. The other 6 nests were substantially made; their distances from the ground: 17, 20, 22, 24, 28, and 33 feet, respectively.

March 17. Here there were 8 new nests, as well as numerous old ones. The new ones were all within 150 feet of one another. One of them was very small, carelessly constructed, and had but little padding. It was low, 19 feet from the ground.

April 4.   In the first of these trees there was an atypical nest; it consisted merely of a mass of leafy twigs and branches on top of a very wide horizontal branch.   There was little shape to the mass, but apparently it had been slept on.   One nest in the second tree seemingly had not been used.   It had a slightly convex rather than the typical concave form, and it was angular rather than rounded.   The branches near this nest were not strong (the wood seemed to be rotten) and several of them had broken off, probably during the process of bending to make the nest. I assume that an animal had started to build here but had found it impracticable, and had thereafter constructed a nest in another place.

No nest examined was soiled by excrement.   In a few cases, as appears from the above notes, partly eaten fruit was found in the nest or dangling over its edge.

The process of nest building is a rapid one.   On one occasion, when conditions were such that an animal could be clearly observed from the time that it started until it finished its nest, the performance was timed; it took three minutes.   The animal was deliberate in its movements, apparently being in no rush or hurry to complete the task.   On another occasion, a chimpanzee interrupted its work so often, climbing about in the tree, "visiting" other animals, and eating, that the process was spread out over 25 minutes.   When light is insufficient for visual observation, the speed of nest building cannot be determined, auditory data (breaking of branches) being without value for the purpose since different individuals of the group start work at different times.   The animal at work stands about where the middle of his nest will be and reaches out in one or another direction to grasp a branch and bend it towards him.   His feet, apparently, are used in holding branches in place and perhaps assist in the crude weaving process.   In my observations, the chimpanzee always stood above his nest, never working at it from below. After finishing his nest, the animal did not lie down immediately, but stood or sat up in it for a time, in one instance leaving it to go to another part of the tree for a few minutes.   As far as I could determine, the chimpanzee sleeps in his nest lying on his back or side; legs and arms are probably drawn up close to the body.

Before starting work on nest building the members of a group wander about in the trees selected as a resting place for the night for several minutes. Whether such selection is the duty or privilege of a leader, or whether it is a matter determined by common assent, I was not able to ascertain. I never saw any indications of dispute over a particular site in the nesting tree. No evidence was obtained indicating that more than one animal worked on a given nest or that one individual built a nest for another animal.

Chimpanzees were never seen occupying nests during the day. I saw animals feeding and resting in trees containing nests, but never getting into a nest during daylight hours. Chimpanzees do rest and, perhaps, sleep, at various times of day (especially around noon), but they do not use nests for these siestas. If they did, they would be exposed to the direct rays of the sun—an exposure which, as has been pointed out before (page 30), these apes sedulously avoided. The conditions pertaining to diurnal rest or sleep are described, in another connection, on pages 32–33.

There appears to be no reason for doubting that the chimpanzees which I observed always slept in nests at night, and that, with the exception of infants, each animal had its own nest. The evidence supporting this supposition is rather strong, if not entirely conclusive. A frequently used method of work in the bush, as described earlier (p. 15), was to have scouts locate the sleeping place of a group of animals, and to be near that spot before daybreak the following morning. Following this procedure, we were on hand when chimpanzee groups awoke in the morning on 23 different occasions. In each case, there were at least as many— often more—nests in the trees as there were chimpanzees known to be present, infants excepted. These apes were never found during the first hour of morning without at least an equal number of new nests—known to be new by criteria discussed below and by the fact that fresh excrements were found underneath—being discovered nearby. I saw only 8 individuals, in three different groups, actually getting out of their nests in the morning, but this fact is attributable largely to the circumstance that it was usually too dark, when the animals arose, to make such observa-

tion. When a group of animals was under observation at dusk, (eight instances) one always heard, even if one did not see, the breaking of branches, as nests were being built.

February 15. At 5 P.M. the sun disappeared behind the hills. . . . . About 5:30 we started in the direction towards which the chimpanzees had disappeared. At 6:05 I saw a chimp swinging in a tree, about 300 feet ahead. I stopped short. Was in high grass with a clump of bushes directly behind me. At that instant a flock of about ten very large birds, which we evidently had disturbed in the bushes, went flying and screaming away, towards the chimpanzee. The latter disappeared in the foliage of the tree. A minute or two later I saw a good-sized animal (young adult?) on low branch of a tree, right beside a nest. Was it still working on construction of nest? The cracking of branches could be heard coming from the same general direction, but apparently further back. (The animal under observation now was about 200 feet distant.) The chimp got into the nest and then looked about, especially, it seemed to me, in our direction. I thought I saw it wave its hand in a peculiar way, then and twice later. Communicational gesture? No cries. Evidently the animal sensed that there was something wrong. It got out of nest and ascended a little in tree; then, standing on both feet and hanging by both arms, it literally peered in our direction. After looking thus for at least 3 minutes, the animal got back into its nest but, al-thought it was pretty dark by this time, I thought I could still see it peering at us from the nest. The sound of cracking branches, further back, continued until 6:30. My natives said they had seen a total of 5 animals; I saw only the two mentioned. At 6:40, when all was quiet and it was completely dark, we reversed our direction and quietly crept away.

It seems entirely probable that very young animals, perhaps 12 pounds and under, do not construct nests, but sleep with adults. We cannot be certain, however, that the larger individual is always, or even usually, the mother of the infant, although one might well expect this to be the case. One observation, made late in April, gave rather clear evidence that sometimes, at least, young animals do sleep with adults. This day's work resulted in the capture of Kambi, a young female weighing less than five pounds, who was brought back to America; we quote at some length from our notes.

April 27.   The night before, two of the boys came back, reporting that they and Sango had found a group of chimpanzees and that they knew just where the animals were sleeping. . . . . We got up at 1:15 A.M.; the big tom-tom was sounded.   About 20 natives had responded by the time we got started, a little before 2 o'clock.   Before we left the road, this number had been doubled.   A moonless night.   On the eastern horizon lightning was flashing frequently.   A mile or so from our desti-nation we found Sango and two boys sleeping by a log fire; we aroused them.   Travelled E NE on the road as far as possible and then struck northward into the bush.   Had two oil lanterns with us.   When about one-third mile from our destination we dropped most of the baggage and put out the lights.   The last stretch was hard going—up and down hill. . . . . At least four times I had explained to my guides exactly what they were to do: Find the nests where the animals were sleeping and the next morning, before it got light, form a circle of men around those trees. . . . . After the fiasco of the day before I had shown them that even if the chimpanzees had been where they were supposed to be, we would have had no success, for we marched right *under* the trees, instead of forming a circle around them.   On this morning the same thing hap-pened.   The animals were there, true enough, but our guides led us directly under the nest trees.   We arrived there at 3:55.   Two minutes later we heard chimpanzees descending.   At least one animal descended from one tree and two animals from a group of three trees 100 feet south. When I realized what was happening, I decided that our only chance to keep what animals remained up was by starting noise and burning grass immediately.   I therefore fired the Winchester, which was the signal for the hubbub to begin.   I kept on firing and everybody yelled and beat trees with clubs.   Just a few seconds after the noise started, there was crying of a baby chimpanzee in an isolated tree.   I ran to the spot; several natives were already there.   With the flashlight I saw a very small animal hanging by one arm from a small branch, not very high up. The natives were standing around with clubs, ready to knock the animal on the head when it dropped.   The tree was not very straight and was easy to climb.   I mounted and grabbed the infant by the back of the neck.   We wrapped it in the robe of one of the blacks; it continued to squeal for half an hour, and frequently thereafter.   Five grass fires had been started; the noise-making was continued.   About 5 A.M. it began to get light.   There were no animals remaining in the trees.   Thunder and lightning increased; clouds began to pile up.   The following deter-minations were made: In a large tree, from which I had heard at least

one animal descending, there were six nests. In the clump of trees to the south there were five nests. A little to the E was the isolated tree from which I had picked the young female chimpanzee. *It contained only one large nest.* It is obvious, I think, that the infant had slept with an adult; the latter had descended rapidly, being in great fear, and the little one had lost its hold. The adult had not stopped to pick up the infant, who was thus left in the tree, hanging by one arm, as we found her. About 75 feet to the west was a large tree with four nests. That makes 16 nests in all. They all looked very new to me, but the natives claimed that half of them were at least 24 hours old. In descending, the animals had not made any vocal sounds, but at least some of them had been noisy in descending. That the animals had departed to the large, heavily wooded valley to the north, there is little doubt. It would be very difficult to find them there, especially after they had been alarmed. It began to rain. We left the spot at 6 A.M., heading for the shelter of a nearby village. Our approach there was noisily heralded by the squealings of the baby chimpanzee.

Since my work was conducted during the dry season, I had little opportunity for getting first-hand information on the nesting or sleeping habits of chimpanzees during the season of rains. One observation, however, supports the statement of my native helpers and of several Colonists, that these animals sleep in nests at all times of the year. On March 23rd it rained from 6:20 until 9 P.M. My notes for the following day strongly indicate that a group of animals had slept in nests that night.

March 24. By 4:50 we were established in a well sheltered place, some 250 feet from the nesting tree. At 5 A.M. it was just beginning to get light, and it was then that we first saw and heard the apes. First there were sounds of animals moving around in the trees and then could see branches being swayed. There were several short, not loud, anger cries. About 5:10 I saw a medium-sized animal squatting near the periphery of one of the trees, eating. It stayed in that spot about 2 minutes. A little later I could see shadowy forms moving around in two trees; they were feeding, but moved about frequently. As it got lighter I caught glimpses of other animals; there were no less than five and probably one or two more. . . . . Quite suddenly and without any warning, the animals descended and were gone. I am quite certain that they had not seen us and that we had made no audible sounds. (5:30 A.M.) There

had been no vocalization on the part of the chimpanzees other than that one cry at about 5.   Where the animals had been there were 7 nests in two closely adjacent Naray trees.   Nests apparently very new; one examined closely certainly had been used.   Under the trees found four fresh fecal deposits. . . . . Also found small quantity of colorless urine.

The question as to whether each animal builds a fresh nest every evening, or whether nests a day or more old are sometimes utilized, is difficult of solution.   Some of my native helpers held dogmatically to the former alternative, while others were equally sure that occasionally nests are used more than once.   The problem may be attacked from several directions.   1. The life of a chimpanzee nest is not indefinite.   By "life" we here mean the identifiable character of the nest.   Its deterioration starts with the browning and falling off of the leaves and ends with decay and fall of the broken branches which constituted the foundations of the structure.   The first phase of this disintegrative process becomes definitely noticeable, in the dry season, by the third or fourth day.   My native assistants claimed that they could differentiate a nest 12 hours old from one 36 hours old.   The length of time necessary for deterioration to the point of unrecognizability varies—from one to five months, I should say.   Let us posit an average lifetime of two months, and make the further supposition that there were, on the average, 25 animals within the total territory which I covered.   If each animal builds a new nest every night, then, we should have within the specified territory, at any one time, 1500 old and new chimpanzee nests.   This is not an impossible figure, in my estimation, although during my entire stay I counted only about 450 nests, without knowingly ever tabulating the same nest twice.   2. I never saw an animal "adopting" an old nest.   On the other hand, whenever animals were under observation at dusk, evidences of nest building were always present (cp. page 47).   3. Very often (23 instances), the same group of closely adjacent trees contained nests some of which were obviously older than others; in about one-half of these cases the older nests were still green and in good condition.

This fact, while suggesting that the chimpanzee does not usually utilize a previously constructed nest, allows of no sweeping conclusion; the second group of animals nesting in the region may have been larger than the first, and the older nests may not have fallen within a narrow age-range which chimpanzees may require for nests which are to be used a second time.   4. That chimpanzees do not *always* avail themselves of nests already present, even if these are quite fresh, is indicated by the finding, on several occasions, of as many new nests, in a group of trees containing numerous slightly older nests, as there were animals in the band which nested there the preceding night.   5. One observation, repeated in its essentials four times, favors the view that nests *may* be used more than once: One or more leafy branches, obviously fresher than the rest of the structure, were found on the upper surface of the nest where they had obviously served as an extra padding.   Such branches usually had come from the same tree, but in one case apparently had been brought over from an adjacent tree; in each instance this added padding was loose, that is, was not attached to the main branches supporting the nest.   That these extra branches or twigs did not merely happen to get on top of the nest by "accident" or in the course of play activity, is shown by the fact that they bore unmistakable signs of long continued pressure, pressure exerted by the sleeping animal, without doubt.   This observation, in addition to indicating rather strongly that nests are at times used more than once, points to the preference of the chimpanzee for sleeping on fresh leaves which probably are softer, make less noise, and give greater warmth to the nest than do dry leaves.   Such preference would, in turn, furnish one good reason for the apparently common procedure of building a new nest each night.   (A more generally accepted explanation, I believe, is that the ape seeks to avoid his nocturnal enemies by changing his nesting place daily.)   Although the occasional finding of loose, fresh twigs and branches on top of older nests strongly suggests that a chimpanzee nest may, now and then, be used more than once, it is possible that only sick, injured or aged animals utilize such structures, and that even in these cases the matter is importantly determined by such fac-

tors as lateness of evening, sites available for new nests, and excellence of those already at hand.[5]

5. FOOD AND FEEDING

Adequate knowledge of foods eaten by chimpanzees in the wild state has both practical and scientific value. Every laboratory using live chimpanzees, whether its interests are psychological, medical, physiological or morphological, is confronted with the task of keeping its animals as far as possible healthy and normal. There is no more validity in attributing to the genus chimpanzee types and limitations of behavior found in under- or mal-nourished individuals, than there is in comparing the effects of a pathogenic organism on such apes with those on human beings given an adequate diet, and attributing the difference found to characteristics differentiating the two types of organism. It is not an easy task to keep apes alive and healthy, and in this matter diet probably plays an important, perhaps a dominant, rôle. Several of the animals which I brought back from Africa were afflicted with intestinal parasites, which they doubtless picked up before capture, but these parasites had no serious consequences until after the apes had been restricted to a man-determined diet for several months. Some of the things eaten by wild animals probably act as drugs, curative of their diseases. In the absence of specific knowledge concerning the foods eaten by animals in the wild state, our laboratory diets are evolved by a trial and error process, in which errors are costly, and by analogizing from human requirements, which may be, for all we know, very different from those of infra-human organisms. Chimpanzee muscle, for example, must in some way be different from human muscle, for it is obviously much stronger, pound for pound, than the latter. It may well be that such a difference in tissue necessitates, also, a difference in diet, in order that all parts of the body may be maintained in healthy normal condition. Certainly it is easily conceivable that knowledge of the diet of wild chimpanzees may throw light on the behavioral, physiological, and

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[5] A summary of previous studies of chimpanzee nests and nesting habits, especially those of Reichenow, is to be found in *9*, p. 226.

perhaps even structural differences between man and his most closely related forms.

With the possible exception of locomotion, our observations of feeding were more complete than those made of any other category of behavior. Although the chimpanzee is ever cautious, always on the alert for signs of approaching danger, he is apparently less so while feeding, especially when really hungry, than at other times. The food which he finds in the bush, at least at the time and place of my observations, is not of concentrated caloric value; large quantities of it must be ingested, and consequently the process of eating occupies a fair share of the animals' waking hours.

Evidence obtained concerning the kinds of food eaten by the chimpanzee is direct and indirect. The former was derived from observation of animals engaged in eating. Such observations were made with the unaided eye and, frequently, through binoculars. Indirect evidence, of strictly limited value, in compiling our dietary list, was of three principal types: a) Examination of feces. Some of the materials found in the excrements evidently had passed through the alimentary tract unchanged or not changed beyond the point of certain identification. So, for instance, my first proof that the chimpanzee eats palm nuts came from the finding of this food, almost completely intact, imbedded in feces. More often than the finding of such unequivocal evidence was the discovery of seeds or stones in the excrements which could not be identified with any degree of assurance as coming from a given fruit. b) Very often in places that had been visited by a group of apes, as attested by nests or excrements, there would be found fruit trees with signs of the animals having eaten there: partly eaten fruit, stones of the fruit, broken branches or pieces of the shell or skin of the fruit. Such evidence, of course, could be accepted as valid only if corroborated in other ways. c) Reports of the natives concerning what they had seen chimpanzees eat constitute poor evidence, since I found, on more than one occasion, that the native has a rather different notion of the nature and virtue of truthfulness than has the European. But such statements had their value in drawing my atten-

tion to possibilities which subsequent investigation sometimes verified.

We list and describe, below, 34 different foods which, it was ascertained with certainty, at least some chimpanzees of French Guinea eat during the months of February, March, April, and May. Unfortunately, we have the scientific names of but a few of these. That the natives should know the scientific designations certainly was not to be expected. Furthermore, I was unable to find any negro who knew the common French names for any of the plants furnishing these foods. Indeed, there are probably few European colonists in French Guinea who know more than a few of the names applying to this African vegetation. I had hoped to get considerable aid from a book by Pobéguin (7) found in the library of Pastoria. This volume lists a large number of plants growing in French Guinea but gives most of the native names in Malinké dialect, mentioning the Sousou name in only a few cases. I found several natives who claimed that they understood Malinké, but they were not able to help me, since their knowledge of the two dialects or languages did not include botanical terms. Only seven plants described by Pobéguin could be certainly identified with items in my list. During the first two months in the bush, I carefully collected specimens of each new fruit or plant found to be eaten by chimpanzees, and stored these specimens, wrapped in waxed paper, in a box in my hut at Nérébili. Late in March a family of field rats made their home under the floor of the hut and, in addition to carrying off unbelievable quantities of food stuff, destroyed most of my specimens. Those which were left, I found, had dried up or had otherwise deteriorated beyond recognition. Not being adequately equipped to preserve such materials, I made no further attempts to collect specimens, and brought back with me only verbal descriptions and drawings.

In the following inventory I give, first, the name of the item in Sousou. There follows, in parenthesis, the name spelled according to symbols given in Webster's Unabridged Dictionary (G. & C. Merriam Co., 1928), so as to indicate as accurately as possible the native pronunciation. The natives do not write their mother

tongue; it is a spoken language only, as far as I was able to determine. Thirdly, there is given a description, exactly as I wrote it in Africa with the specimens before me, of each item. In those few cases in which I found the plant listed in Pobéguin's "Essai," I append the description there presented. The name of the month in which the plant was seen being used as food follows. Finally, after the word "Frequency," there is recorded the number of times that I got undeniable evidence that the particular item was being eaten by chimpanzees; each unit in these "Frequencies" represents at least one, and usually from three to ten animals, from whom evidence was secured at one time. One or more plus signs following this figure indicates roughly the amount of additional indirect evidence obtained.

1. Yentenyi. (Yĕn tĕn' yĭ) Diameters: 8'' and 6½''. Green skin. Pulp consists of tough white fibers radiating from center to surface; these pulp fibers are slightly astringent. The fruit as a whole has fruity, pleasant odor. Embedded in the pulp are numerous seeds, having a slippery, glutinous, thin but tough skin. Diameters of seeds: ½'' and ¼''. The kernel of the seeds have a nutlike consistency, almost tasteless. Grows near water only. Animals chew the pulp and spit out wads of fiber from which juice has been expressed. Swallow seeds? April. Frequency: 1+

2. Dundareh. (Dûn' dă rĕ) Spherical. Diameter: 1¾''. Brownish green, rough skin. Pulp is solid, white and tough. Many minute seeds in long, narrow pockets radiating from center to the adherent skin. Has a pleasant, fruity odor. The pulp is astringent and a little bitter. April. Frequency: 1.

3. Seekunyeh. (Sē kûn' yĕ) Diameter: 1¼'' and 1''. Brown, rough skin. Pulp light green, mealy and sweet. Skin and pulp less than ⅛'' thick. Large stone in center. Has odor like that of an apple. February. Frequency: 1+

4. Suhgeh. (Sû gĕ) Diameters: 1⅛'' and ¾''. General description like that of Seekunyeh. Grows in dry places. February. Frequency: 1

5. Bomenti. (Bō mĕn tē') Diameters: ⅞'' and ⅝''. Skin orange-brown to brown color, is tough and thick, has velvet-like surface. Inside the skin are two to four seeds covered with mucus-like substance, extremely tough, long diameter ½''. The glutinous covering is sweet and pleasant;

fruity. The seeds are bitter and astringent. Grows in clusters. March. Frequency: 3+

    Pobéguin:

        Bembé-Oignan (Malinké)

        Famille: Anacardiacée

        Nom Scientifique: genre Odina non déterminé

        Diametre de tronc: O metre 60

        Hauteur de l'arbre: 15 metres

        Baie comestible

        Fruits: grappes de petites baies veloutées jaunes ou orange, comestibles, existe en plusieurs variétés

6. Wehyenyi. (Wĕ yĕn′ yē) Size and shape of a large lemon. Thick green skin, quite tough, exuding a milky, sticky fluid. The pulp is white, sometimes light tan, but quickly turns red upon exposure to air. It is extremely astringent but has little taste. The pulp contains a large number of white nuts, tender in small specimens, tough in larger ones. Grows on woody vines, close to the ground. The skin is not eaten. April. Frequency: 1+

7. Kanti (Kän′ tē) Diameters of a small specimen: 2″ and 1½″. The skin is thin like that of a plum. Yellow to light red in color. The pulp has the same consistency as that of a plum but is more stringy and adheres tightly to the stone. The stone is very large, in a small specimen having a long diameter of 1½″. The fruit is extremely sour and astringent when yellow. As it reddens it becomes distinctly sweet although still tart. Used by chimpanzees when yellow, by natives when reddish. Tastes exactly like gooseberries. Grows on fairly large trees in dry places. April. Frequency: 2++

8. Foray. (Fŏ rā) Spherical. Diameter: 1¼″. Thin yellow skin which peels off easily. Pulp is white, glutinous, very sour and astringent Contains two large stones. Grows on woody vines which produce a rubber-like substance. April. Frequency: 1.

    Pobéguin:

        Apocynées. Landolphiées. Lianes. Landolphia Heudelotii. Liane à caoutchouc, Poré en Foula, Foré en Sousou, Gohine en Malinké.

        Fruit: boule jaune à maturité de la taille d'un petit citron, contenant des graines, dures, irrégulieres, entourée d'une pulpe mucilagineuse, acide, comestible; donne un excellent caoutchouc.

9. Whoray. (Hwŏ rā) Diameters: 1⅜″ and 1⅛″. Shape of a pear.

The thin adherent skin is green, at the time this fruit is eaten by the chimpanzee; later it becomes reddish. The pulp is white, tough, and contains a sticky, milky fluid. In the center is a mass of very small white seeds. It has a pleasant fruity odor. March, April. Frequency: 3

10. Gerenyi. (Gĕ rĕn' yē) Oval shaped. Diameters 1″ by $\frac{7}{8}$″. Has a large hard stone, which the animals often swallow. Inseparable skin and pulp are green, very astringent and slightly fruity in odor; bites and looks like an olive. Grows on large trees, always near water. March. April. Frequency: 5+++

Pobéguin:

> Myrtacées, Eugenia (sp. non déterminé) Ko-Kissa en Malinké. Arbre de 10 à 15 m pousse en terrain humîde et au bord des rivieres, fleurs plus petites blanc jaune; fruit: petite baie violet foncé, non comestible.

11. ? Nuts of the palm tree. Diameters: $1\frac{1}{4}$″ and $\frac{7}{8}$″. Has very large stone which is covered by a thin layer of reddish, oily fibers, which are sweet and of pleasant taste. Grows at the top of palm trees, usually near water. March. Frequency: 1+

12. Naray. (Nâ rā) Pods which grow in clusters or bunches. A specimen pod is $10\frac{1}{2}$″ long, $\frac{3}{4}$″ wide, $\frac{1}{4}$″ thick and contains 19 seeds. Hull of the pod is thick, fibrous and green; is not eaten. The seeds, like flattened peas, are tough; usually swallowed but rarely chewed by the animals. These seeds are embedded in a yellow to orange colored filler, which is mealy and sweet and has the consistency of potato flour. Grows in dry places on fairly low trees, having light green needle-like leaves. Natives use the yellow filler for making a sweet drink. They grind the seeds to a flour used in cooking. March, April. Frequency: 7+++

Pobéguin:

> Néré (en Malinké)
> Famille: Légumineuse mimosée
> Nom scientifique: Parkia biglobosa
> Diametre du tronc: 1 metre
> Haute de l'arbre: 25 metres
> Grand et bel arbre, tres commun dans toute la Guinée. Fruit: paquets de gousses de O m. .30 de long renfermant des petites graines, entourées d'une pulpe farineuse, sucrée, jaune, mangée par les indigenes. Graines oleagineuses contenant 47% de matieres grasses. Ces graines, fermentées dans des trous à eau

et ensuite torréfiées, servant à faire une graisse de cuisine (nommée Soumara), très prisée des indigenes mais désagréable aux Europeéns par son odeur nauséabonde.

13. Tumbeh Naray (Tûm bâ Nâ rā)   Similar in appearance to ⌗ 12, but always grows near water on large trees.   The pods are thicker than those of Naray and contain only about 10 large seeds.   The yellow filler is scantier and is bitter rather than sweet.   Tumbeh Naray is never touched by the natives.   April.   Frequency: 1+.

    Pobéguin:

        Ko-Néré (en Malinké)
        Famille: Légumineuse
        Nom scientifique: non déterminé
        Diametre du tronc: 0 m. 80
        Hauteur de l'arbre: 25 metres
        Grand et bel arbre poussant au bord des rivieres.   Feuillage fin, composé bipenne vert foncé.   Fruit: grosse gousse épaisse, longue de 20 à 25 cm. genre de l'Ovala du Gabon (Pentachletra Macrophylla).

14. Bonkwey. (Bōŋ kwhĕ)   Spherical, diameter: 3″.   The skin is green and encloses a greenish-white pulp $\frac{5}{16}$″ thick.   The pulp is juicy and sour; it exudes a milky, sticky fluid and contains a number of white seeds $\frac{1}{2}$″ long.   Grows near water.   April.   Frequency: 1.

15. Youenyi. (Yo͞o ĕn′ yē)   Round berries.   Diameter: $\frac{1}{2}$″.   Light green velvety skin, enclosing some white pulp and a mass of very small seeds.   Little taste or odor, slightly astringent.   April.   Frequency: 1.

16. Koloya.   (Kŏ loi′ yà)   Diameter: 2″ and 1$\frac{1}{2}$″.   Surface color is green when unripe, yellow when mature.   Eaten by chimpanzee in both stages.   The skin is thick and tough and is characterized by crater-like elevations and depressions; unpleasant taste.   The pulp is white and slightly sweet.   It contains 10 to 12 triangular seeds, diameter $\frac{3}{8}$″ and $\frac{1}{2}$″.   The seeds are hard and bitter; they have no odor.   As the fruit gets ripe the pulp becomes fluffy and quite sweet.   Grows on trees 15 to 20 feet high.   February, March.   Frequency: 2+.

17. Souyinyeh (So͞o yĭn yĕ).   Heart shaped.   Diameters: 1″ and 1$\frac{1}{2}$″. The skin is green at time chimpanzees eat the fruit, later it becomes yellow.   The pulp is white, slightly astringent and has little odor.   It contains 30 to 40 small white seeds.   April.   Frequency: 1.

18. Laboý. (Là boi)   Long brown pods, 12″ long, 2$\frac{3}{4}$″ wide.   The shell is fibrous, leather-like.   It contains 5 seeds, each 2″ square.   These seeds have a thin, light brown skin and are very astringent and slightly bitter.

The fruit has no odor, it grows only near water. April. Frequency: 1.

19. Lingeringeh (Lĭŋ gĕ rĭn′ gĕ) Diameter: $3\frac{1}{2}''$ and $3''$. Has a fold like a peach. Green in April, later becomes yellow. Has a thick skin enclosing a white spongy pulp, about $\frac{1}{2}''$ thick, containing a thick sticky white fluid. In the center, separated from the pulp by a thin hard white membrane, is a white spongy material (diameters: $2''$ and $1''$) containing about three black seeds. Diameter of the seeds: $\frac{3}{8}''$, $\frac{1}{8}''$ and $\frac{1}{4}''$. April. Frequency: 1.

20. Bunyeh. (Bûn′ yĕ) Diameters: $3''$ and $2\frac{1}{2}''$. Has a thick tough hull, bright red in color, and marked by a shallow crease along one side. This hull contains a cavity which is filled with a thick, stringy, cloudy fluid, which has no taste and which surrounds five smaller fruits, cylindrical in shape. Diameters: $1\frac{1}{2}''$ and $\frac{3}{4}''$. These smaller fruits have a thick yellow skin and a pleasant fruity slightly glutinous sweet pulp, light yellow in color. Inside the pulp there is a longitudinally paired seed. This seed has first a very thin brown skin and then a red skin; it is crisp and solid, but has little taste. Always grows near the water in high trees. April. Frequency: 1.

21. Dundahlee Wenyenyee. (Dŭn′ dä lé Wĕn yĕn yē) Diameters: $2'' \times 1\frac{1}{2}'' \times 1\frac{1}{4}''$. Grayish-green, thick, very tough skin, which has an opening in the bottom covered by five-sectioned flap. The white, hard pulp is fairly sweet. It contains about 12 purple seeds, diameters $\frac{5}{8} \times \frac{1}{2} \times \frac{3}{8}''$, which have the consistency of a walnut and are astringent and bitter. April. Frequency: 1.

22. Leebiree (Lē′ bĭ rē) Grows in clusters like grapes. One stem, $6''$ long, bears 30. Diameters of each: $\frac{1}{2}''$ and $\frac{5}{16}''$. Green in April, becomes light purple later. The skin is like that of a grape. The pulp is green, sweet-sour, fruity, pleasant. In the center is a relatively large, hard stone. April. Frequency: 1.

23. Sahkwee (Så kwē) Grows on the side or end of a stem, occasionally a leaf grows out from the distal end of the fruit. Green fuzzy skin. The pulp is green and slightly sour, has a fruity odor. In the center is a large, hard stone. April. Frequency: 1.

24. Sahrahree. (Så rà′ rē) Diameter: $\frac{3}{8}''$. Light green, fuzzy skin; almost no pulp. In the center is a mass of very small seeds. Grows in dry places. April. Frequency: 1.

25. Kundi (Kûn′ dē) "Fromager" in French. Cylindrical pods about $5''$ long. The shell is tan or light yellow in color. Very hard and thin; difficult to open. Inside the shell is a wooly, fuzzy stuff containing large numbers of small white and pink seeds. These seeds are tender and sweet. Grows near water. February. Frequency: 2.

26. Mawkeh. (Mŏ kĕ) Grows in clusters. Diameters: $\frac{3}{4}''$, $\frac{5}{8}''$ and $\frac{3}{8}''$. The relatively thick, brittle shell is black and has a velvet-like surface. Inside there is a thin green skin and then a white pulp. In the center is a small tender kernel, slightly astringent. February, March. Frequency: 2+.

> Pobéguin:
>> Ko-Fina en Malinké
>> Famille: Légumineuse césalpiniée
>> Nom scientifique: Dialium Guineense
>> Diametre du tronc: 0 m. 35
>> Hauteur de l'arbre: 15 metres
>> Arbre moyen assez commun au bord des rivieres, est peu droit
>> et de feuillage clair. Fruit: petite gousse noire ronde, veloutée
>> a une graine entourée d'une pulpe farineuse acide, comestible.

Kuhkwee (Kû kwē) is probably another variety of this fruit. It is described by Pobéguin as follows:

Verbénacées, Vitex, (spec. non déterminé) Coudou en Malinké, Koukoui en Sousou. Arbre moyen de 8 a 10 m., commun dans toute la Colonie, feuilles digitées lisses, petites fleurs mauves en corymbe a l'aisselle des feuilles; fruit baie noire comestible, existe en deux variétes.

27.   ?   A round disc-like fruit, the margin of which is composed of fine green bristles. The center portion swells to a thickness of about $\frac{1}{2}''$. This fruit is almost tasteless, slightly astringent. It is eaten when green, later becomes brown and falls to the ground. February. Frequency: 2.

28.   ?   Grows in thick clusters, each berry about $\frac{1}{4}''$ in diameter. The green shell is relatively thick but tender. The center is green colored and soft. It is slightly sweet but very astringent, is eaten by monkeys as well as by chimpanzees. March. Frequency: 1.

29. Singkunyi (Sĭŋ kûn′ yē). A green plant growing about 4 or 5 feet high. At the top is a white flower. The stalk, $\frac{3}{4}''$ in diameter, has a sweet and succulent center which is eaten by the chimpanzees. April. Frequency: 1+.

30. Leaves of small palm trees. The inside of stalks or stems has a pleasant, slightly sweet juice. Chimpanzees chew this and later eject the woody fibrous pulp. March. Frequency: 2+.

31. Stalks of the banana plant. The inside of young banana stalks consists of thin, white sheets of vegetable matter formed into a vertical roll. There is also some fuzzy, porous stuff. All of this is very tender and juicy, a little bitter and astringent. The green bananas of these plants were not touched by the apes. March. Frequency: 1.

32. Pink and brown blossom. In the center there is a small capsule containing almost tasteless fluid. The chimpanzee puts a great quantity of these small cup-shaped blossoms into his mouth and later ejects the fibrous matter. March. Frequency: 1
33. An unidentified and undescribed pinkish white blossom. March. Frequency: 2.
34. Leaves of young saplings and small bushes. Astringent, bitter. April. Frequency: 2.

Table 1 summarizes some of the more obvious characteristics of the thirty-four different foods. The figures represent the frequencies with which, by direct evidence, the various foods were found to be eaten. The first of the two totals at the bottom of each column shows how many of the 34 items manifest the special characteristic. The second total is weighted according to the frequencies shown in the above inventory, and should therefore give a better estimate of the relative frequency with which the several attributes enter into wild chimpanzee diet.

Certain facts must be kept in mind in considering the data of Table 1. 1) Although we may say with assurance that chimpanzees do eat the 34 foods listed, at certain times and places, we must be cautious in drawing further conclusions from the figures. The number of our observations of feeding is relatively large when compared to those found in earlier reports, but it is not large enough to warrant the application of statistical treatment. The differences in frequencies, therefore, while highly suggestive in some cases, cannot boast of statistical reliability. Opportunity for chance errors of sampling is obviously great when the total number of cases is only 56. Selective factors, also, probably were operative; the Naray, for instance, grows on hillsides where observation is considerably easier than it is in the heavily wooded valleys where the Gerenyi grows. Although I am convinced that Naray and Gerenyi formed two of the staples of chimpanzee diet during the period of my work, I should be unwilling to venture even a guess as to which of these two was eaten in larger quantities or more frequently. 2) My observations were made within a relatively small area, within a minute fraction of the totality of African territory inhabited by the chimpanzee. It is

TABLE 1

SERIAL NUMBER	FREQUENCY	SOUR	BITTER	SWEET	FRUITY ODOR	PLEASANT	UNPLEASANT	MEALY	ASTRINGENT	OILY
1	1				x	x			x	
2	1		x		x	x			x	
3	1			x	x			x		
4	1			x	x			x		
5	3		x	x	x	x			x	
6	1								x	
7	2	x		x	x	x			x	
8	1	x			x				x	
9	3				x				x	
10	5				x				x	
11	1			x		x				x
12	7			x		x		x		
13	1		x						x	
14	1	x							x	
15	1								x	
16	2		x	x			x		x	
17	1								x	
18	1		x				x		x	
19	1		x		x					
20	1			x	x	x				
21	1		x	x	x				x	
22	1	x		x	x	x				
23	1	x			x					
24	1									
25	2			x		x				
26	2	x		x					x	
27	2						x		x	
28	1			x					x	
29	1			x		x				
30	2			x		x				
31	1		x			x			x	
32	1									
33	2									
34	2		x						x	
Totals....	56	6	9	15	14	12	3	3	20	1
Totals weighted by frequencies..		8	13	28	23	23	5	9	33	1

only reasonable to suppose that as the vegetation in different parts of Africa varies, so also will the diet of animals living there vary. Quite possibly, of course, the general type of diet is the same, or similar, regardless of the specific locality in which the animals happen to be living.   3) Fruits as well as other forms of vegetation are seasonal.   Many, if not most, of the foods which I have listed came to edible maturity and disappeared again within the period of my observations.   In the rainy season the character of the food is probably quite different than it is during the dry season. It appears probably that the number of fruits available decreases appreciably after the rains commence and that the animals became more dependent upon other kinds of food stuff.   From what was told me by natives and some of the French colonists, it appears that the chimpanzee eats green stuff, especially leaves, in great quantities during the rains, and that towards the end of the rainy season he gorges himself with Menghi, a small grain which grows wild and which is also used by the natives as food for themselves and for domesticated fowl.   4) It is extremely doubtful that all of the 34 items of food listed were being eaten throughout the period of my work.   Some of them, it was obvious, were shunned by the animals until after they had reached a certain degree of ripeness.   It is quite possible, I believe, that some of the things consumed by the apes were eaten only of necessity—because nothing better, nothing more suitable, was available.

My notes contain scant mention of fruits found which are not, as far as I could determine, eaten by the chimpanzee.   Several of these, the natives warned me, are poisonous.   Some others I found to have very disagreeable tastes.   Although I did not attempt a complete survey of all fruits growing in the territory in which I was working, and a separation of these into those eaten and those not found to be eaten by the chimpanzee, I carried away the impression that at least one-half, if not more, of the total number of fruits found were being used as food by the apes.

As seen from above inventory, 28 of the 34 foods listed can be classified as fruits, 3 as stalks or stems, 2 as blossoms, and 1 as leaves.   I had neither direct nor indirect evidence that the animals ever ate roots or tubers.   Some of my native helpers stated

dogmatically that the chimpanzee never goes under the ground for his food; others gave me vivid descriptions of how these animals yank roots out of the ground or dig for the wild sweet potato. (This potato, incidentally, requires long and complicated treatment before it is edible for man.) Although I witnessed digging behavior on more than one occasion and later examined the holes, there were, in no instance, any signs that the animals had been digging for food; it was apparently a form of play. Especially in view of what was said above, such negative evidence should not be given undue weight. The district in which I was working abounds in wild bees. Natives, who themselves make frequent raids on stores of the sweet substance, told me that chimpanzees are very fond of wild honey and eat it frequently. No direct evidence supporting this contention was found, and the only indirect evidence pointing to its probable correctness may be seen in the extract from my original notes for March 4th quoted on page 80.

Concerning *animal* foods of the chimpanzee, my observations give no clear answer. I have seen the apes, reclining in trees and on the ground, make quick hand and arm movements, *as if* they were catching insects or other small animals, and then bring hand to the mouth. This type of behavior occurred, usually, while the animals were relatively inactive. My observations provided no evidence that chimpanzees catch and eat larger animals, such as birds, rodents, and fish, or that they eat birds' eggs. I consider it entirely possible, however, that they do use any or all of these organic foods without my having seen them do so.

It is interesting to note (see table 1) that the most frequently found characteristic of chimpanzee diet is its astringency. Important consequences of this property on the eliminative process as well as on oral hygiene and general health seem very probable, especially in the light of correlations, discussed below, found to exist between the eating of non-astringent foods and diarrhoea. The relatively high percentage of bitter and sour (often unripe) foods may have influences on the health of the animals which will have to be verified, tested, and explained experimentally. A number of the fruits eaten by chimpanzees before they were ripe (as well as later) have, to the human sense, either lack of taste or

even an unpleasant taste before they reach full maturity, whereas the animals seemed almost to prefer them while they were still unripe and, consequently, often sour, astringent and hard. Among these may be mentioned especially Kanti, Foray, Whoray, and Koloya.

The amount of food stuffs ingested by an adult chimpanzee during a 24-hour period must be large. To form a reasonably accurate estimate of this quantity would require the close and constant obvservation of one or more animals for at least 12 to 14 consecutive hours, and if one were interested in the net amount of food used by the chimpanzee, such uninterrupted and careful observation would have to be supplemented by collection and examination of products eliminated during the same period. Although we have no exact answer to the question under consideration, we may get some rough ideas on it from the data available. Firstly, we know something of the amount of time spent by the chimpanzee in the feeding process. One of the first things the animal does in the early morning, and one of the last things he does late in the afternoon, is to eat. Observations supporting this statement were frequent and unequivocal. In the morning, feeding usually started as the first light of day became visible, and continued with relatively short interruptions for from one to three hours. The period between 7 and 11 o'clock in the forenoon was usually divided among various activities, of which feeding might claim about one hour or less. From eleven in the morning until three or four o'clock in the afternoon activity of all kinds was rarely of an energetic nature, and the feeding that occurred during that period was desultory and seemed contingent upon what happened to be around in the immediate vicinity. While the sun was high and the sky unclouded, the only feeding I observed was in trees of heavily wooded valleys, where there was ample protection from the direct rays of the sun. During the last two or three hours of daylight, activity was increased and feeding was resumed with fresh vigour and appetite. This sketch, of course, is to be understood as a sort of "composite picture," of which the several elements show wide variations. It suggests that the chimpanzee spends a net period of between three and five or six hours in feed-

ing. During some of that time, say the first half hour of each of the two main feeding periods (early morning and late afternoon), the animals eat greedily and without the finer discrimination shown when they are less hungry. During these ravenous periods, seeds, stones, and skins are swallowed and no particular selection is made between ripe and under- and over-ripe foods. As the intensity of hunger subsides the speed of eating decreases, while discrimination, selection and rejection increase. So, for instance, the ground under a Naray tree, where animals have been feeding in the middle of the day, will usually be strewn with the hard, tough "peas," found inside of the Naray pods, which the animals spit out after swallowing the sweet yellow-orange filler; if, on the other hand, the animals have been feeding in a similar tree very early in the morning, such rejected "peas" will not be found under the tree but may be seen later, totally undigested, imbedded in the excrements. I have seen chimpanzees, dozing lazily in the crotch of a tree, half-heartedly stretch out an arm and bring food to the mouth which was sometimes eaten and sometimes merely nibbled at. At other times I have seen them in frenzied haste, gulping down food as fast as they could stuff it into the mouth. Although the volume of food stuff which is ingested during three or four hours of serious feeding may be great when compared to the volume eaten in a day by man, it is entirely possible that the quantity is insufficient to maintain the animals at near their potential maximal weight. The food-value concentration of the fruits, stalks, leaves and blossoms eaten is probably so low[6] that we should perhaps compare the digestive process of chimpanzee with that of Herbivora rather than with that of Omnivora who use much meat. Consideration of our observations on the excreta of chimpanzees may cast further light on the quantitative problems of food consumption.

[6] In this connection I should like to suggest a possible explanation for coprophagy, so frequently seen in captive animals. May not this behavior find its explanation in the fact that our laboratory diet, being relatively concentrated, fails to satisfy the animal's digestive system, which has been accustomed to handling a large volume of food stuffs? That is to say, may it not be that coprophagy indicates a need for more bulk in the diet? I never saw any signs of this filthy habit in the wild chimpanzees which came under my observation.

Feeding behavior has already been described as sometimes showing tremendous avidity and at other times manifesting itself as a disinterested, frivolous or playful activity. In either case, the methods of bringing food to the mouth are about the same. Sitting, squatting, reclining, or standing on a limb of the tree, one hand grasping the same limb or a neighboring one (usually one overhead), the animal reaches out to the fruit with his free arm. Then he may pick or strip the fruit off of branch or twig and unload the handful of food into his mouth; this manner of feeding occurred frequently when the fruit was small. Or, he may break off a branch or twig and then either strip the fruit from the limb (frequently using his second hand in the process), or, dangling the severed twig before his mouth, eat directly from it. The limb, instead of being broken off, may merely be bent until its load of fruit is close enough to be stripped off with one hand or directly with the mouth. Although the feet were frequently employed to bend, break, or hold a branch, I never saw them used to convey food to the mouth. Various combinations of the three general methods outlined were often observed, and changes from one method to another were introduced in accordance with the demands of the situation.

February 23. Nearer to us than the trees until now the focus of our observation, was a large clump of smaller, bush-like trees. Here I saw, a few minutes later, a large mature male. There were other animals in the same clump of trees, as attested by the violent movements of branches, but I saw only the one. He was eating Koloya. . . . . These plums were eaten in two ways: 1) Sometimes they were plucked off with the hand, and then conveyed by the hand to the mouth; 2) sometimes a twig bearing the plums was drawn towards the mouth, and then the fruit was bitten off directly. The animal usually sat with one hand holding to a higher branch. But once this chimp travelled a short distance in the tree upside down, and, in the same position, ate of the plums. He descended to the ground after three or four minutes by going to the end of a branch which was pulled down almost to the ground by his weight. When the branch was released it sprang back with a swishing sound.

February 26. A large adult chimp was feeding in a tall Gerenyi tree. He was near the top of the tree, moved around quite a bit, and often

stretched and reached out very far for the fruit, striking some beautiful poses.   Usually plucked the fruit with one hand, sometimes the right and sometimes the left, and then brought it to the mouth.   Occasionally bent twigs towards his mouth.   For several seconds was using both hands in eating, supported only by his feet.

Some fruits were swallowed whole, some were well chewed and certain parts ejected.   Some were peeled or shelled and only the inner parts consumed, while still others were eaten as we eat an apple, certain portions being rejected.   Where animals had been feeding on the inner part of palm leaf stalks (the leaves were broken or torn from the trunk), on Yentenyi, or on the brown blossoms, the ground for some distance around bore wads of woody substance which evidently had been spit out after the juice had been expressed.   Usually the animal remained in one place until most of the fruit within reach in all directions had been eaten or at least had been looked over and rejected.   Then he moved to a new location in the same or another tree.

February 13.   At 6:15 A.M. we heard the panting cry of chimps.   The valley is moderately wooded; a small marigot flows through it.   The sun appeared at 6:10 and ten minutes later I saw four large chimps in a tree which was half yellow, half green.   All but one descended very soon and the one exception stayed less than 15 minutes. . . . . At 6:45 I saw five small or medium-sized chimps in a completely yellow tree; they were eating the yellow blossoms.   The characteristic method: sitting on haunches or standing on feet, hanging from higher branch by one arm, they brought the twigs bearing blossoms to their mouths with the free hand, eating directly twig to mouth.   The animals were distributed fairly evenly over the tree.   There appeared to be no competition for the food, and there was no order of eating, left to right, top to bottom, and so on.   The animals moved about the tree very little; it seemed that they were able to get much food while remaining in one position. . . . . By 7:15 the tree was fairly well stripped of its yellow burden and one by one the chimps descended out of view.

When a group of animals started on a general movement away from the feeding place, some individuals often broke off a larger or smaller branch, well laden with fruit, and dragged this with them, eating as they went.   Frequently a branch was broken off and

then dropped to the ground. Probably on closer inspection the fruit had proven to be undesirable. Natives told me that chimpanzees sometimes toss food down to members of their group who, because of age, sickness, injuries, or advanced pregnancy, are unable to climb trees easily. This story has all the earmarks of a typical native yarn. I was not able to disprove it, however, for the high grass usually present made it quite impossible to see if there were any animals below. The only evidence favoring the story was the finding, on two occasions, of neat little piles of Naray "peas" which had been ejected directly under Naray trees in which animals had been feeding. It is quite out of the question that such piles were formed by animals dropping the stones from branches of the trees. On the other hand, it is entirely possible that a strong and healthy animal had finished his meal on the ground.

Drinking behavior was observed only twice, one animal each time. Crouching at the bank of a tiny stream, hands and feet on solid ground, the ape lowered his head until his lips were in contact with the water. The water was apparently sucked, rather than lapped up. This position was maintained not over 15 seconds, and then the animal left. Although the large number of small streams in this country afford almost constant opportunity for drinking, the indications are that chimpanzees require and use very little water. It seems probable that environmental factors, such as humidity and nature of food, may have something to do with this.

The excrements of chimpanzees were found on practically every occasion that the animals were seen. Quite rarely were they found, however, in the absence of other evidence indicating the recent presence of the animals. That is to say, old fecal deposits, more than 24 hours old, were seldom encountered. This fact is doubtlessly to be explained by the warm, moist climate of French Guinea, which favors the rapid disintegration of such material. Probably the failure to find even skeletal remains of wild animals, including those of the chimpanzee, is partially accounted for in the same way.

Under trees in which chimpanzees had slept fresh fecal matter

could invariably be found the following morning. Defecation
probably takes place early in the morning, very soon after the
animals leave their nests. For, although the nights are cool in
French Guinea, it is improbable that some disintegration would
not be noticeable after ten or twelve hours. Certainly it is true
of captured animals that defecation occurs regularly in the early
morning and rather rarely just before bed-time. Quite often I
was able to identify the number of fecal deposits under the nest
tree with the number of nests above.   While the close correspond-
ence between the number of nests in the tree and the number of
fecal deposits under it in the morning indicates that at one time of
day (about sunrise) all animals defecate, the eliminative process
during the rest of the day seems to be a more individual and
variable affair. For fecal deposits might be, and were, found
almost any place where the animals had been: under fruit trees,
on trails which recently had been traveled, near streams where the
animals had stopped to drink, and on their rest- and play-grounds.
But except for the ground under the nest tree, deposits were rarely
found in numbers in any one place.   As far as I am able to deter-
mine from a complete survey of my notes, each animal must
defecate at least twice and probably three or four times a day.

The size of a single deposit varied from one-half ounce (liquid
measure) to as much as eight or ten ounces.   In color the excre-
ments varied from a dark olive green or reddish brown to light
shades of green or tan, with almost all intermediate shades and
hues represented. In consistency and texture the feces also
differed considerably. When defecation had taken place while
the animal was on the ground the deposit was usually formed,
i.e. the mass was firm enough to retain its twisted shape.
Feces dropped from the branches of the tree and falling to the
ground were without "shape." Really hard feces were never
found, but most of them were more firm than those usually found
in captive animals. About March 20th Naray came to edible
maturity and was available in the bush until the middle of April.
This fruit is extremely common in the vicinity in which I was
working,[7] and is apparently very well liked by the chimpanzee.

[7] "Nérébili" means approximately: Under the Naray trees.

It is one of the few foods which is distinctly mealy and has a sweetness which is "sugary" rather than "fruity." As contrasted with many other foods of the wild chimpanzee, Naray is not astringent. Almost from the very day that the animals began to eat Naray I noticed a distinct change in the nature of the feces: they became thin and almost watery. This condition continued for a week or ten days and then gradually subsided until, by the end of the Naray season, the excrements were about as firm as before. Aside from this ten-day period when nearly all of the feces found were much thinner than usual, signs of diarrhoea were few, and some of these could be definitely correlated with a situation provoking fright.

In texture the excrements were usually stringy, fibrous. About eighty per cent of them contained undigested (not necessarily undigestible) substances: "peas" of the Naray, whole palm nuts, stones and seeds of Bomenti, Kanti, Gerenyi and of other fruits.

February 24. Around the trees where the animals had fed and on the trails leading from them, we found many deposits of fecal matter. They all contained stones of the Gerenyi, and in some cases the outer green hull was still firmly adherent to the stone, i.e. undigested.

Sometimes the stool was packed with such substances, there being just enough true fecal matter to hold the mass together. In other cases the percentage of such materials was quite low, ten to thirty per cent by volume. Although by no means always, the nature of several fecal deposits found at about the same time and place, as under one or a group of nesting trees, was usually similar as regards firmness, texture and color.

I was able to observe the process of defecation only three times, twice early in the morning when the sun was not yet visible and the animal was on a branch of the nest tree, and once when we suddenly and unexpectedly came upon two animals feeding in a tree, both of whom defecated as they rapidly descended. The latter feces were much thinner (bordering on a diarrhoeal condition) than others found the same day. In the laboratory, it is a common observation that the chimpanzee defecates freely and often to the point of diarrhoea when he is fright-

ened.   When defecating while on the march, the chimpan-
zee apparently does not stop his progress for performing of the
eliminative function, for the excremental mass may spread for
some distance on the trail.   While defecating from the nest tree,
on the other hand, the animal appears to exercise care in having
the excrements drop to the ground.   This appears both from the
two cases of direct observation and from the fact that I never
found branches of nest trees soiled by feces.

Urine was not found as often as were feces, which fact is easily
explainable by the difference in the nature of the two substances.
If the urine had fallen on green leaves, especially glossy ones,
these usually retained traces of it in the form of markings, prob-
ably mineral residue after evaporation had done away with the
fluid.   Although the nature of the ground underneath the nest
trees was often such that no traces of urine could be found, we
may be reasonably certain that urination occurs almost univer-
sally in the early morning, for almost without exception, where
the ground under the nest trees was favorable, covered with low
bushes or with large dead leaves, one could find either urine itself
or traces of it.   On some days the urine found was practically
colorless, at other times it was dark brown in color.   Whether
colorless or darkly pigmented, it was always clear (not cloudy),
and never had a strong odor.   I was not able to correlate color or
its absence in urine with any other observed fact, such as composi-
tion of diet, but I did notice that in all cases but two, the color of
urine for all members of a group (or all of them for whom speci-
mens were found) at a given time was approximately the same.
Needless to say, I convinced myself in various ways that the pig-
mentation was not derived from the leaves on which the urine had
fallen; that it did not result from exposure to air is indicated by
the observation that colorless urine did not become pigmented
before it entirely evaporated under natural conditions.   The facts
already cited make it plain, furthermore, that colored urine did
not come exclusively from menstruating females.

6. SOCIAL BEHAVIOR I

One of the most interesting phases of chimpanzee free life, that
having to do with social relationships, is unfortunately extremely

difficult of study. Isolated bits of behavior pertaining to this interest may be observed, but these cannot be pieced together, as is the case to a much larger extent in feeding and nesting activities, to form a reasonably complete story. If, for example, one is concerned with the question of monogamy versus polygamy, in the wild chimpanzee, identifiable members of the same group would have to be observed during sex activity a larger number of times. It is difficult and rare enough to observe copulation at all; to keep a group under continuous observation for a week or a month, and, at the same time to be able to identify or distinguish the individuals composing the group, present difficulties which I did not even approximately overcome. Contributing importantly to the difficulties of sociological study is the fact that most of the pertinent behavior occurs on the ground, where observation is always difficult.

As already noted in the section on social organization, the chimpanzee group is composed of from four to fourteen animals, which may include several mature males and females, together with a large proportion of small animals. One animal, in five instances identified as a male, stands out from the rest of the group by his superior size. In five cases it was observed that the largest animal was in the lead when a general movement of the band was in progress; in one instance a large female took the foremost position. Evidence that the group is dominated by a despotic leader is lacking. On the other hand, none of my observations contradict the native tradition of such an autocratic rule. It seems quite clear, however, that certain individuals have recognized precedence over certain others. This matter appears to be determined largely by size. When a large animal wanted to pass a smaller one on a branch, for instance, the latter showed obvious attempts to make room for the former, but if this position was reversed, the smaller ape found some roundabout way to his destination.

March 19. At 5 A.M. it was just beginning to get light. We could hear the animals before we could see them. Were 200 feet to their south. First heard chimpanzees climbing about in the trees a few minutes after 5. Then could see branches jerking up and down. Several minutes

later there was a short, medium loud anger cry, and immediately there-after I saw an animal (5–6 years old?) squatting on a branch, eating. From the trunk side of the branch came a larger animal; the first one immediately grasped a nearby branch and swung away, clearly in order to get out of the way of his superior. When the latter had passed, the smaller animal resumed this original place, but left again in a minute or two. . . . .

Various incidental observations which might be interpreted as bearing on the problem of leadership, appear below and in earlier chapters.

Although the region of my activities has a rather dense animal and human population, chimpanzees seemed to come into direct contact with their neighbors only rarely. Probably this state of affairs is to be attributed in part to the cowardice of chimpanzees and in part to the timidity of other animals, but mainly to the general tendency of all inhabitants of the bush to mind their own business. Small monkeys were often found in close proximity to chimpanzees, but always seemed to retreat from an approaching group of the larger primates. On two occasions there was in-direct evidence that baboons, relatively fearless and aggressive animals, and chimpanzees were fighting.

February 14. Started in northerly direction at 4:40 A.M. At 5:30 we came to our destination; first light of sun just visible, although moon was still shining. In a heavy clump of trees by the marigot could hear chimpanzees making a great racket. Seemed to be fighting. Inter-spersed with chimp vocalizations there was barking which did not sound like chimps; baboons, perhaps? Could dimly see forms moving about in trees. I advanced slowly. When within less than 100 feet, sudden violent movements in the trees; Sango said chimps were descending. Kept up the careful advance. When we got there, chimpanzees were gone, but there was a band of baboons in trees. . . . . We proceeded by indirect route to place where Sango thought the animals had fled. . . . . His guess was correct, and after we had marched for about 30 minutes we saw chimpanzees feeding in a group of trees, near margin of a forest and grass field. I saw 10 animals. . . . .

The lone female chimpanzee which I had under observation on March 20th (see page 19) showed obvious fright reactions when a

large hawk-like bird circled within a few yards of the tree in which she was stopping. The fear apparently concerned herself rather than the problematical infant hidden in the bush below. One evening we had the following unusual experience at the village of Nérébili.

February 22. At 7:10 P.M. I was in my hut, when I heard the panting cries of chimpanzees nearby. At the same time the dogs of the village barked loudly. I went out. The apes were apparently at the marigot, less than $\frac{1}{2}$ mile north of the village. It seemed as if the chimps and dogs were hurling defiances at each other. This was repeated three more times; the last time at 7:40. Vocalization of dogs and chimpanzees in each instance appeared to start simultaneously.

Towards man, the wild chimpanzee is usually very timid, less so, apparently, in thickly wooded places than in open areas. Increased density of vegetation may partly account for the reported decrease in timidity during the rainy season, although the fact that there are few natives abroad in the bush and that there is practically no capturing and shooting at this time, probably has something to do with the difference. Chimpanzees appear to possess a remarkable ability to distinguish between human behavior which is directed towards them and that which concerns other things. The last quotation above illustrates an occurrence repeated, with variations as to detail, several times—the building of nests within easy earshot of a native village. In one instance I observed chimpanzees feeding in a tree not more than 250 feet from where a dozen native women were noisily washing clothes, talking and laughing. The women paid no attention to the animals and the latter seemed totally unconcerned about the natives. But if chimpanzees caught us *looking* at them from much greater distances, they immediately took to flight. Once we came upon a group of the apes feeding some two hundred feet to one side of a frequently used native trail. As an experiment, I instructed my natives to keep on walking and not to look at the animals. A few of the latter descended, but most of them stayed where they were. Had we stopped to observe them or in any way direct our attention to them, they would have immediately taken alarm, as

was demonstrated forcibly in many otherwise similar situations. The European conductors on the trains running from Conakry to Kankan tell of seeing chimpanzees and chimpanzee nests a hundred yards or less from the tracks, undisturbed by the noisy, smoke- and spark-belching locomotive or by the natives leaning out of the windows of the cars. One afternoon I was making a study of nest structure and had mounted a tree for the purpose. My native helpers were stretched out on the ground below, sleeping or dozing.

February 15. At 5 o'clock I heard a noise, and looking up, or rather down, I saw, not more than 50 feet from the base of the tree in which I was working, 5 chimpanzees, two of which were very big, the other three medium-sized. A big surprise, it seemed, for all six of us. I had been working rather quietly just before they arrived, making measurements and writing in my note-book, and they apparently became aware of my presence at the same moment that I saw them. They were hesitating, all looking up at me. On all fours throughout the brief period that I saw them. It was now too dark, on the ground, to take a picture, so I simply watched them quietly. They stayed perhaps 30 or 40 seconds, and then turned back and in rather leisurely fashion went the way they had come. They made no vocal sounds. . . . .

Had our positions in this situation been reversed, that is, had we approached the animals, instead of they us, the result probably would have been different. As it was, the animals showed practically no fear. The chimpanzee is curious, and that curiosity extends even, or should we say, especially, to objects considered dangerous. Even when retreat was hasty, the animals gave us a sharp though brief examination.

April 8. Came to Koniokori about 8 A.M. Entered the steep, jungle-like valley to the east, and soon heard a short salvo of chimp cries. We rested near the stream to await the next cries to direct us. They came within a few minutes and were quite close, to our east. Advanced a short distance; the extremely heavy undergrowth and dark shade made it difficult to see far. Saw an adult chimpanzee sitting on the ground, head and shoulders leaning against a rock, right arm extended, grasping a woody vine. Evidently the animal was dozing. But for some reason

it soon departed. Then, a little to the north, I saw a large animal, white-faced. It saw me, apparently, and peered intently in my direction. (There were no chimp cries after I had seen the first animal.) In order better to see me (?) the chimp moved up and down, to the right and left, and even moved away some drooping branches which obscured the view. It disappeared, but a minute or two later reappeared, and looked again. Dimly saw another animal nearby. Then both disappeared for good, and we were unable to locate the group again.

February 21. We went to the south of the tree, following one of the many chimp trails, and entered an area of grass, low trees, and bushes. Wandering about, trusting mostly to luck, came to a place where I could see several chimpanzees moving on the ground, away from us. . . . . We followed as well as we could, several times catching glimpses of animals still walking away from us. They were on all fours. Then, most unexpectedly, we came in sight of a brown plum tree, in which a single animal, a male, was feeding. Naturally we had made some noise in approaching, but evidently he had not heard us, for he kept right on eating. It happened that he was facing away from us; we threw ourselves on the ground, but were only partially screened by the grass. Watched him thus for 6 or 7 minutes. . . . . Seemed to be a young mature male, dark-faced. . . . . Then he began to climb higher in the tree, and in doing so, his glance fell on us. He stopped, peered at us for an appreciable length of time, head and neck stretched forwards, and then, without uttering a sound, climbed down the tree, swiftly but not in a panic. We pursued in the direction he had taken. Again we stopped short, perhaps 100 feet from a clump of trees and bushes in which, at first, there were 3 or 4 animals. All but one disappeared almost immediately. The one remaining was black, enormous. He kept peering at us, first from one place, then from another. The two places from which he made his observations were about 15 feet apart. Was on all fours in going from one to the other, but while he looked he stood upright, hands holding to the trunk of a tree or overhanging branch. Went back and forth four or five times, and then disappeared. In all of this activity there was not a sound from the animals.

According to native reports, chimpanzees fight with man only when they are being attacked and cannot flee. On such occasions they defend themselves savagely and effectively. (See the description of Schweinfurth and of Buck, quoted by Yerkes and Yerkes, 9, pp. 229 and 230).

Although, as we have seen, the chimpanzee appears as a thoroughly peace-loving animal so far as his neighbors are concerned, his domestic life seems to be a succession of quarrels and squabbles. In view of the fact that most of the evidence on this point was auditory, rather than visual, we should perhaps modify the above indictment by saying that the dissension among these apes may sound worse than it really is. It may be that chimpanzees, not unlike some higher primates, make much noise about trifles, and so give the impression of being more quarrelsome than they actually are. Certain it is, however, that the naïve observer, judging solely or mainly from the sounds—vocalization and drumming— which he hears in the bush, gets the impression that the intragroup relations of wild chimpanzees consist principally in contention and strife. Judging from observations of captive animals, especially from the unpublished observations of Dr. O. L. Tinklepaugh, it seems probable that chimpanzee noise-making sometimes has sexual significance.

The playfulness of captive chimpanzees, especially of young individuals, has elicited comment from almost every investigator working with these animals. (See especially the comments of Koehler, cited by Yerkes and Yerkes, 9, p. 254.) The inventiveness of young apes in creating new games and their ability to amuse themselves for hours on end with some object like a piece of rope, a stick or a nail, which is used in a great variety of ways, are truly amazing. The wild chimpanzee seems to spend no less time or energy in play activity than does his brother in the laboratory. Whether he is equally ingenious in discovering new forms of play would be hard to say. I consider it quite possible, however, that in the bush, where there is plenty of room for liberation of excess energy in gross activity, the animals may exhibit such inventiveness less markedly than they do under conditions which largely inhibit activities like running, jumping and swinging about in trees. In this connection we may recall that the cause of two objectionable forms of behavior found in captive animals, masturbation and coprophagy,[8] has been attributed by some to the boredom,

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[8] For a different explanation see page 66.

the lack of changing and novel stimuli, afforded by a laboratory situation.

The following account, which touches on various phases of chimpanzee behavior, includes a description of a favorite sport of the animals: playing "tag." On March 4th, I had a group of chimpanzees under observation early in the morning. After feeding for an hour, the animals had descended to the ground and had disappeared to the north. I examined the territory where they had been eating and where they had slept that night. Then we proceeded northward in the hope of again finding the animals.

March 4. We ascended to the plateau and had not gone far when, before us and to the left, we heard a brief chimp cry. We proceeded and suddenly, 7:30 A.M., heard chimps descending nearby and galloping away. The tempo of the sounds was very much like that of a horse's gallop. Went to where the sounds had come from and found plenty of evidence that the animals had been feeding. . . . . Porters insisted there was no use in attempting to follow, that the animals had been alarmed and would not allow themselves to be surprised again. We continued search. Following one of many chimp trails through high grass, we heard and glimpsed animals descending a tree not more than 1000 feet from the feeding trees which we had just left. 8:30. Several times we heard chimps running on ground. Followed as best we could; progress through the high grass very difficult. Came to a spot clear of grass but containing a number of small trees. Under one of these we sat down, not knowing in which direction the animals had gone. At 9 A.M. there was a short fear-pain cry. Then there was repeated four times, at rather short intervals, a beating or stamping sound of time values: – – and – . – At 9:25 saw two animals in a very leafy but not fruit-bearing tree about 150 feet to our S. Animals perhaps 6 years old. Could not determine sex. They were obviously playing tag. Back and forth in the tree they went sailing, the rule being, seemingly, that they must remain in the tree. When one animal was cornered the other one reached out an arm, touched his play-mate and then swung away with lightning rapidity. No closer bodily contacts. Most regrettable that I could not take movies of this, but light was too poor; foliage very thick. This play continued with slightly decreasing intensity for 20 minutes, at the end of which time the two animals descended to the ground. About ten minutes later there was a tremendous racket— vocalization—from our W. Fear, pain, rage, anger, and excitement

seemed to be expressed. The noise lasted for a full minute or longer. Three minutes later I could hear chimps galloping towards us. I was ready with the Eyemo. They passed us, 3 of them, at about 20 foot intervals, not more than 30 feet to our S. Did not notice us. But the high thick grass again made photography impossible. I might have got a few dark spots moving through the grass, no more. A little later, 10:30, we saw chimps in a group of 4 trees about 250 feet to the E. We were still parked under clump of small trees, pretty well hidden by the high grass. The trees in which the animals now were bore thick foliage. Included one of the flat disc fruit and one tree overgrown with a vine which bears a pinkish blossom which the animals ate during the course of this observation, 10:30 to 2:40. During all of this time the chimps remained in or very close to this group of trees. Until 12:15 there was quite a bit of moving about, ascending and descending the trees, climbing about, movements in the grass and nearby bushes. Also some vocalization; sometimes of the "fighting" kind, several times merely short grunts or barks. There were at least four animals; perhaps as many as six. I designate the four by letters. A was a large female, dark faced, probably the mother of B, a small animal (2 years old or less), whose arms and legs seemed very long and head big in proportion to size of trunk. C and D were medium-sized animals. D had very light face. When I first saw the animals A was sitting on a branch, with B climbing around and over her. Then C came and seemed to hand something to A, after which C carefully licked his hands. This act was not repeated. For a time A and C were sitting close together and once they put their faces together. (C definitely smaller than A). In the meantime B was clambering about, but always stayed pretty close to A and came back to A frequently. For a few minutes C showed marked attention to B, stroking, scratching or tickling the little one. For a time B was at the breast of A, but whether the little one was being nursed, I do not know. . . . . C descended and reascended several times. D seemed to be on the ground part of the time and sometimes in another part of the same tree. . . . . Noticed that A was more slow and deliberate in her movements than any of the other animals. I saw the youngster, B, swinging by his arms for a minute or more at a time, twice. Also C. After about 12:30 P.M. the animals became more quiet; they dozed a great deal. A seemed to prefer lying on her side on a branch, steadying herself with one arm stretched out towards a neighboring limb. While lying thus, she would sometimes scratch her head and stretch. This was also true of C. C's favorite resting position seemed to be lying flat on his back on

a branch, one hand, two feet, or hands and feet grasping a horizontally parallel but slightly higher limb. The animals seemed to be resting, rather than sleeping, for there were frequent movements, such as scratching, stretching, changing position slightly, and so on. Once I saw distinctly how C yawned. There was also desultory eating; while in resting position C would grasp some of the "green disc fruit" and put it into his mouth, or he would draw a twig of this fruit towards his mouth and bite some off directly. Seemed to be more play activity than serious feeding. Once, while C was resting, B moved a leafy branch up and down so that this touched C and finally aroused him. Whether there was premeditation in this, whether it was a sort of teasing, is naturally impossible to say; certainly it looked like it. . . . . 2:45 P.M. The animals had silently disappeared.

Playing tag was observed on several other occasions, but in no other case was it seen to continue for more than three or four minutes. I never saw more than two animals at a time participating in this game.

Another sport, frequently engaged in, was wrestling and play-fighting. Elements entering into the game were rolling on the ground, pulling at the other's legs or arms, biting which was sometimes sufficiently severe to elicit a cry of pain or fear, pushing, and running away and chasing. Especially when the animals were close together, the play was accompanied by audible breathing or panting, such as one hears when dogs are playing strenuously. Perhaps the ticklishness of chimpanzees contributes to their enjoyment of this form of play.

Few cases were observed in which objects of the environment were employed as toys or playthings. One instance, although imperfectly and incompletely observed, merits description. On March 24th we had seen a group of animals leaving their nest trees in the early morning and then feeding, nearby, for about 30 minutes. We lost sight of them for a time, but later, guided mainly by their cries, were able to pick up the trail again. They were evidently on the move northward, stopping briefly, here and there, to feed. There was some indication also, that they had stopped to drink at a small stream. At about 9:30 A.M. we came to where the animals were evidently making a longer halt.

There, in a not very well shaded spot, we saw two animals, one perhaps eight years old, the other about five, playing on and around what appeared to be a pile of leafy branches. They were rolling on it and several times went a little distance and then ran towards the heap of green stuff, flinging themselves on top of it. The observation lasted less than two minutes. Evidently we were seen, for quite suddenly the animals disappeared in the direction away from us. We approached and examined the ground where the chimpanzees had been playing. The play object proved to be a sort of basket, bottom side up, made of thick, woody, caoutchouc vines. Very clearly, these vines had been twisted and roughly woven to produce this particular formation. It certainly was not a natural physical Gestalt. The possibility that the basket was made by other animals or by man, may, I believe, be entirely discounted. Also, in view of the many natural shady resting places all about, it seems most improbable that the basket was constructed primarily as a sort of sun-umbrella and was only incidentally used as a play object. The structure had great resiliency and springiness. These characteristics are, of course, inherent in the type of construction, but may be emphasized by the use of rubber vines.

The instances of play behavior thus far cited illustrate a point noted in all of my observations, namely, that such behavior is almost entirely restricted to fairly young animals. I never saw an individual identifiable as full-grown engage in play activity. While this may be a rule which has many exceptions, it probably indicates a tendency towards lessened playfulness as the animal passes into adulthood. The play of very young animals is mild and gentle (see pages 80, 86).

April 15. The second animal was a small one; face medium dark, sex not determined. Probably about two years old. At first it was high in the tree where I could not see it. When it descended to the level where the female was, it did not eat but played about, especially hanging by its arms and swinging on a branch, very close to the large female. It also climbed around on the branches a great deal. The third animal was medium sized. It was resting, eating and playing gently with the youngster.

As the animal grows older his play becomes more energetic. The tremendous amount of vocalization and tree-beating heard in the bush gives rise to the conjecture that play gradually gives way to more or less serious fighting and perhaps to sexual expressions accompanied by sound.

The newly born chimpanzee is probably quite as dependent upon parental care as is the human infant, although complete dependency no doubt lasts over a longer period in the case of the human baby and child. The infant ape apparently possesses one exceedingly important ability from early in life, that of clinging with its hands and feet to an adult. In the bush, I never saw an infant being literally carried; the larger animal supported its burden passively, the youngster hanging by its own efforts. During locomotion the infant is horizontally upside down, since it clings to the ventral side of the adult. Hands and feet of the small animal grasp the side or back hair of the adult between hips and arm-pit. On the ground this manner of transportation is probably safe enough, but high in the tree-tops it sometimes appeared very precarious for the infant. A large chimpanzee was usually slow and deliberate when carrying a baby, and before making any violent movements often gave the young one warning.

April 15. Then we went still further south and saw 5 or 6 animals in several Naray trees. They moved around quite a bit. . . . . One of them (black-face?) had a baby at the breast. All animals moved about so much (they were not in a hurry to retreat but obviously were nervous about the situation) that I am not sure that this same animal finally descended with the baby. The animal which did carry away the infant was very large, and instead of climbing down the tree as the others did, it climbed outwards on a branch and dropped, with the infant at its ventral side, to a lower branch, out of sight, of the same or another tree. Moyla and I then entered the jungle and very soon saw a dark form travelling above in the trees. Most fortunately it stopped exactly opposite a little opening in the leaves, so that I could see it. It was a large female, no swelling of genital region. An infant was at her ventral side, both arms wrapped around the mother's torso between arms and hips. The hands of the babe, I know, were holding on to the hair of the mother's back or side, and I am quite sure that this was the case with the feet also.

The mother definitely a black-face, the infant definitely a white-face. Not sure whether the mother animal saw us or not; probably she suspected something. Stayed in the same spot some 3 minutes but changed her posture (nervously?) several times. Her lips were tightly compressed and twitched. She looked around frequently. Her breasts could be seen but did not appear much larger than those of most animals; nipples definitely protruded. The infant remained quite motionless, tightly pressed to the mother's ventral side, face turned sidewards. The two animals disappeared when the female walked back in the direction she had come from, the infant, arms and legs pointing to the sky, still clinging to her.

March 23. At 5:50 A.M., in a rather small tree, I saw a chimpanzee, seemingly not very large, with an infant at its ventral side. Was not able to determine exactly how the infant held on, but am sure that it held on through its own efforts, i.e. the mother did not hold it except as noted below. The baby chimp had a large, round head; hair color was same as of the larger animal, black. Hair on arms seemed very long, sticking out from body; looked unkempt. The mother was eating white blossoms of the tree. These blossoms were stripped off with the hand, and then hand was brought to mouth. The mother took many positions which seemed dangerous for the infant. She usually was in sitting or squatting position while she ate. The baby would often bend the upper part of its body out from the mother and stretch out an arm as if to grab something, but it never actually grasped anything. (I am assuming that the larger animal was a female and the mother of the smaller one, although I have no direct evidence on either point.) The baby, it seemed to me, must have been hanging on principally by holding on to the mother's hair with hands and feet; possibly it exerted a clamping pressure of its legs on the waist of the mother. The movements of the larger animal were deliberate; she moved comparatively slowly and before each swing to another branch she put one hand on the back of the baby; probably a sort of warning tap.

The ability of the infant to hang on to an adult probably lies principally in remarkably strong flexing strength of the fingers. To a limited extent, if at all, clamping pressure of the legs may contribute to this. The tremendous grip of young chimpanzees is illustrated in the case of the female Kambi, captured in April, 1930. At that time the animal weighed under five pounds, but

could grasp folds of my skin hard enough to cause black and blue marks. Kambi did not walk until the following October.

As the young chimpanzee develops, he depends less and less on older animals to carry him. One of the last things he is able to do in the way of locomotion is to climb up and down trees with thick trunks. That is to be expected, since absolute arm-reach or arm-span certainly is a vital factor in this activity.

April 4. Heard chimp cries not far away. 1 P.M. Moyla and I approached and, led by the cries, suddenly came upon the animals. They were in a heavily wooded marigot valley. In one tree there were three chimpanzees, the first of which descended promptly and rapidly. The other two were a large female and an infant. The latter walked by itself from the peripheral end of a branch to the trunk, and there it fastened itself to the ventral side of the large female who then descended rather slowly and cautiously.

In another connection (page 27), we have seen how a larger animal may partially support a smaller one, not an infant, in walking. One case was observed (page 88) of an adult chimpanzee carrying a two or three year old animal on its back. I suspect that the younger ape was ill or had been injured and, therefore, could not maintain its hold in the usual position.

Although it seems probable that the infant ape is usually carried by its mother, my observations show clearly that this is not always the case.

April 11. At this spot we were close to the animals and could hear their cries and loud TT sounds at frequent intervals. Also could see trees being violently disturbed by chimpanzees. Green monkeys were in the immediate vicinity. Went further south with Sugeta only and entered the marigot jungle. Cries and TT sounds now were to our north. At one point we stopped, where we could see trees being disturbed and could hear branches snapping and cracking some 75 feet to the east. Cries and TT sounds continued to come from the north. The animals near us left soon, going northward. Could hear them walking on the dry leaves. Sugeta and I proceeded northward. When we had gone some 800 feet and were partly up a gently sloping, heavily wooded hillside to the east of the marigot, we heard chimp cries coming from the south,

apparently from the very place we had just left!   From this hill, where
we were well hidden, we could see three animals in the first crotch of a
very high non-food tree standing by itself a little to the east of where the
heavy vegetation stopped.   The crotch was at least 75 feet from the
ground.   One of the animals was a mature female, black-face, genital
area swollen but not very red.   Then there was a medium-sized animal,
dark-faced, probably a male.   The third animal was perhaps 12 or 18
months old, sex not determined, white-faced.   This small animal was
very active in contrast to the others, climbing about in nearby branches
and over the larger two animals, and swinging by its arms.   The larger
animals played gently with the youngster, stroking and scratching him
and handling him generally.   In this the medium-sized male was more
active than the large female.   The tree was about 300 feet away from us;
all observations were made with binoculars.   After I had been observing
thus for about 10 minutes, three chimpanzees passed close by us, not
more than 25 feet away.   They were going south.   One seemed very
large, but I could not see much because of intervening vegetation.
Think they saw us, but am not sure.   Walked fairly slowly.   Some 4
minutes later a large, broad-shouldered male climbed the tree where
aforementioned three animals were resting and playing.   He climbed
up very fast and immediately went over to the medium-sized animal;
there was bodily contact between the two, I am almost sure.   The me-
dium-sized male thereupon speedily descended the tree.   Then the fe-
male went over to the large male, and these two animals got into a dorso-
ventral copulatory position.   But there were no pelvic thrusts on part
of the male.   (Was the male perhaps instructing the female by tactual
means, or was he pointing out a future rendezvous?   Both animals were
facing northwest.)   After holding this position some 30 seconds the
male went over to another branch, taking the youngster with him, and
peering intently in our direction for almost 2 minutes.   Do not think he
could have seen us, as we were well hidden and perfectly quiet.   The
female also looked in our direction for about 45 seconds and then de-
scended, alone.   A little later the male descended with the youngster
at his ventral side, the arms of the younger animal were around the large
male's neck or shoulders.   Right after this descent two chimpanzees
came towards us from the south, stopping suddenly when about 35 feet
away.   They made a right-about face and retreated the way they had
come.   Their approach over the leaf-covered ground had been noisy,
their retreat was silent.   8:45 A.M.   During the next half hour we
heard animals walking on dry grass and leaves near us, and got glimpses
of two of them.   At 9 A.M. there was a short F-P cry to our north.

It is quite inconceivable that very young chimpanzees could construct their own nests. That they sleep together in one nest with a larger animal, rather than alone in nests constructed for them, is indicated by the observations reported in our fifth section. The finding of rather small nests, poorly made, suggests that the chimpanzee starts nest-building at an early age and does not depend upon larger individuals to make his night resting places for him after he has stopped sleeping with an adult. The observation reported on page 37 shows how the infant is introduced to foods other than his mother's milk. Scattered citations from my notes indicate that the entertainment of baby chimpanzees is left largely to immature animals, from about three to nine years old. The adult chimpanzee does not, apparently, bother a great deal with the very young except to protect them and look after their more essential needs. We close this section with an account of ape behavior which, in an anthropomorphic mood, we might call altruistic.

April 7. Before it was light, we heard several not-loud chimp cries and animals moving about in trees. These cries were short, of the panting or hooting variety, and did not sound very emotional. But as it got lighter these cries increased in loudness and in emotionality. F-P cry frequent. Also TT sound. Could hear the animals breaking branches or young trees on opposite of marigot—unusual for this time of day. I left my natives behind and descended the marigot valley alone, armed only with binoculars and Leica camera. Very cautious approach. Fortunately there were many rocks, so I could move along the stream bed with practically no noise. Came to where I could occasionally see animals moving about behind the dense vegetation. Climbing about in trees, breaking branches, cries and TT sounds continued for about one hour. Exactly opposite (at right angles to the course of the stream) to where I crouched there was a fairly clear space up to the top of the south side of the shallow, narrow valley. The top was some 75 feet away. A migration to the E (my left) began, the animals walking along the top of the valley, so that as each one passed the open space above mentioned I got a good but brief (10 sec.) view of it. I counted 12 animals (including two babies which were being carried in typical fashion). Whether any of the animals saw me on this trip I cannot say with certainty. They passed along at irregular intervals; especially

between the first and the next two there was quite a wait—perhaps 5 minutes. The whole parade took about 10 minutes. The animals walked quite slowly, on all fours. Now there was a wait of 15 minutes, without any sound or sight of the chimpanzees. Indirect evidence (see below) leads me to believe that they were feeding during this time. I had about decided to go up to the side of the marigot valley when some of the animals started coming back, going west along the same path before used. First, at short intervals, there came three animals: two adults and one very small animal (not carried). A family group? They did not see me. Six minutes later came 9 more animals, among which an adult carrying a young animal, perhaps 2 or 3 years old, *on its back*. The adult was on all fours and did not appear to see me; the youngster was flat on its tummy, legs and arms straddling the back of the larger animal, head turned towards me. The other animals, including a female with pronounced swelling, and large mature male, all saw me; each hesitated to give me a long stare and then passed on, without increasing locomotion. But then an exciting moment. A large, well-built and beautifully proportioned male—as large an animal as I have ever seen—stopped, having seen me, and with arms resting on a rock, remained thus in an upright position for at least two minutes. He regarded me steadily, without any sign of fear, now and then glancing towards where the animals had come from and where they had gone. No erection. Almost sure he was a black-face, but my memory is not certain on this point. Through the seven magnifications of the binoculars he seemed huge and very close; I did not think of using this remarkable opportunity for taking a picture. Suddenly and without any warning the animal got down on all fours and came rushing directly towards me. I was as good as nailed to the ground. When about 30 feet away he stopped, picked something up, and ran back up the incline. He was carrying a young chimpanzee, perhaps three years old. When he got near the top of the little valley, and about 25 feet from where he had been standing before, he sat down, back resting against a tree, facing me. The younger animal he held on the ground in front of him, between his legs. The large male was obviously breathing heavily. The two chimpanzees soon departed to the west, each on its own feet. I think what happened was this: the young animal had been heading towards me from the west, southwest. It did not see me, and I did not see it, but the large male, seeing both of us, decided that the youngster was headed for trouble. In order to "save" the youngster, perhaps a son or daughter, he had gone down to its rescue. Significant that he did not

instead give some sort of warning signal. No vocalization or other sound-production from either of the animals at any time during period of observation.

7. SOCIAL BEHAVIOR II

In the summary description of techniques used in the field (page 14), I have already referred to the almost unbelievable amount and intensity of sound-production by free-living chimpanzees. It has been illustrated incidentally, also, in descriptions of various other types of behavior. My experience with captive animals had led me to expect a certain amount of loud vocalization and some ground-stamping and drumming, but certainly I was not prepared for the deafening uproar of noises with which these wild apes often broke the buzzing, humming silence of the African bush.

Most of the chimpanzee behavior which I observed is, perhaps, of a relatively unspectacular nature, but certainly their sound-productions cannot be accused of lacking a melodramatic quality. Although the cries and drumming presaged no danger to human listeners, in fact such sounds invariably ceased as soon as the apes became aware of our presence, their very intensity was sufficient to inspire, if not fear, then something akin to excited wonderment. My Sousou helpers were not really afraid of the chimpanzee, for tradition and personal experience had taught them that this animal does not attack man except under extreme provocation; nevertheless when drumming and vocalizing were close by, my guides and porters sometimes trembled perceptibly in spite of themselves.

Verbal description of chimpanzee sound-production is difficult and necessarily unsatisfactory. For an adequate portrayal, use would have to be made of some method of sound reproduction, such as phonographic discs. An observer with thorough musical training, it is true, might record the sounds from one or two animals,[9] but would have a difficult time I fear, in taking down, in musical notation, the din produced by a group of animals, simultaneously vocalizing and drumming. In order to distort impres-

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[9] See Yerkes and Learned (8, part 2).

sions received in the field as little as possible, I confine myself largely to quotation from original notes.

A word regarding terminology. The chimpanzee makes most of his noises, I should guess ninety per cent or more, while he is on the ground. Since, as has been pointed out before, observation on the ground is exceedingly difficult, I rarely saw chimpanzees when they were at their noisiest. My descriptive terms, therefore, refer to the sounds themselves rather than to the behavior which produced them. "Drumming" is used instead of the more specific expression "tree-beating"; although on the two occasions when I was eye-witness to the production of this sound it was made by slapping or beating trees, the many times that I heard the noise without seeing how it was produced seem to make the less definite term preferable. In my original notes, drumming is called "tom-tom sound" (abbreviated to T-T sound), since it so closely resembles the dull beating of native drums. Vocalizations were too varied and complex to be finely classified; only a few rough categories were established:

1. The excitement or panting cry. Like vocalized panting. The most frequently heard type of cry, occurring in many variations of pitch and intensity. Sometimes single tones, more usually in a series. Apparently signified general excitement, but often seemed to have more specific emotional significance, as of fear, anger, impatience or anxiety.

2. The fear-pain cry (abbreviated to F-P cry, in notes). A scream, usually high-pitched, which always seemed to denote fear or pain. (Captive animals have been heard giving this cry when their behavior indicated anger.)

3. Barking. Much like the barkings of dogs and baboons. High or low-pitched, usually loud. Seemed to signify anger, defiance, or exasperation.

4. Whimpering or whining. Usually high-pitched, never very loud. Denoted disappointment, frustration?

5. Food-muttering. A distinctive sound occurring mainly, it appeared, just before feeding. Not loud, "open," like a soft, abbreviated bark. Seemed to indicate satisfaction.

Practically every day of effective field work included the audition of various sounds produced by the chimpanzee. The follow-

ing quotations from original notes are confined largely to periods of observation in which such sounds were the most significant factor.

April 24. At 9 A.M. Fode came, reporting hearing "many chimpanzees" around marigot D. I went there and hid in the jungle near the stream. The animals were to the north, but for some reason my natives were sure they would come southward very soon. At 10 A.M. heard cries to the N NW. At 10:05 there were cries plus TT sounds from the same direction. At 10:15 cries and TT sounds, now closer to us. Then could hear animals walking on dry leaves; they were passing us but were not quite close enough for us to see them through the heavy vegetation. I estimated their number at 3 or 4, not more. When these animals were but a short distance to the S SE of us, there were cries and TT sounds from their direction. As usual, there was the introduction of several soft, low-pitched, slow panting cries, ending with a high-pitched, piercing scream as the TT beats started. The animals that had passed us kept up cries and TT beats at fairly frequent intervals until 11 A.M. They went to our southeast and stopped about 500 feet from where we were; at that spot the jungle has such thick undergrowth that penetration is almost impossible for man. Cries from animals to our N NW continued; the F-P cry predominated. At about 10:30 the latter animals also began to come in our direction. They seemed to be fighting; at any rate, the F-P cry was very prominent. When they were quite close to us, the dispute (?) became especially violent, and altho I could not see the animals, I could tell that some of them, at least, mounted nearby trees. Then heard one animal come running towards us; the formation of the ground was such that the animal was less than 40 feet away when we suddenly saw each other. I could see only from chest upwards. His mouth was wide open (breathing hard?); a very broad face, eyes relatively small. Very ugly. Doubtless an adult. All of the nose part was white, but black around the eyes. The animal regarded me intently for at least 20 seconds. Then it disappeared for 20 seconds, only to bob up again for another good look. All vocalization and drumming ceased about 15 seconds after the animal first saw me. It seemed very still after all the racket that had been going on. After the second disappearance, I could hear movement on dry leaves indicating that the animals were retreating northward. I assume that they took a roundabout course to arrive at the spot where the first animals were still sending out cries and TT sounds. Twice this TT sound occurred without the introductory and accompanying vocali-

zations—very unusual. The last cries were at about 11. Then there was silence until 1:30 P.M. At that time there was one series of TT sounds and cries. At 2 P.M. and again at 2:07 there were two more series of TT and cries. After that, silence. Waited and then searched until after 5 P.M. without seeing or hearing any more of the animals. The tom-tom sounds of today were in uneven tempo, but of fairly even loudness. They were all very loud and sounded as if made on hollow logs. There was no particular rhythm, that I could detect, to the unevenness of the rate of TT beats. . . . .

April 2. Started at 5:40 A.M. . . . . It was about 6:15 when we first heard the chimps; following these sounds we came to the margin of a heavily wooded ravine. No stream in the ravine, but one flowing at right angles to its lower, open end. We were looking down on the tops of the trees which filled the ravine. Could see nothing of the animals, but were informed of their continued presence there for well over an hour by the sounds which they made. The "feeding" cry was given a number of times; it is an open, short, unfinished sound, often heard at the Primate Laboratory at feeding time. One animal sneezed frequently. Then there was a sound which I have not heard before in the field, although I have heard it at New Haven: a semi-continuous sound, like the snarling of dogs when they play and bite at each other. The F-P cry accompanied this sometimes. Short, chopped-off, discontinuous, low-pitched bark several times. . . . .

April 12. Auditory evidence that fighting was going on was abundant. TT sounds were terrifyingly loud. One sequence: Da *Dummmm* Da *Dummmm* Da. Another sequence: 9 beats in equal and rapid succession, of approximately equal loudness. Often a TT beating was introduced by low-pitched but loud panting cries; then, when the TT started, a piercing, high-pitched shriek. Often there were solo F-P cries. Grunting bark frequent.

February 17. Searched for nests, but in spite of much difficult and painful progress through the bush we were unsuccessful. . . . . Soon after starting search heard cries of several chimps; one cry, apparently that of a young animal, started in typical chimp fashion (screaming) but ended in a quivering, trembling (fearful?) note which I never heard before. . . . .

March 5. During this time, also, and continuing until 5 P.M. there was much sound of chimps moving in trees and through the underbrush;

also sight of branches being violently disturbed. At exactly 4 P.M. there was one sudden and very intense F-P cry, one animal only. Cry got fainter, but waxed again several times without entirely dying out in the meantime. Then there were some violent movements in a tree, after which a short repetition of the F-P cry, immediately followed by two different animals giving, in different pitch, briefly, the excitement (panting) cry, which ended this series of sound-productions. At 4:35 there were two differently pitched panting cries, simultaneous. After 5 P.M. no more sounds.

April 16. At 8:15 heard a terrific uproar of chimp cries to the south. Seemed to be mainly the anger cry; do not think the F-P was involved. At the same time there was heard the warning cry of green monkeys. Possibly a dispute between the two species? All was quiet for the next fifteen minutes; then, during the following 5 minutes, four solos of TT sound with usual accompanying cries. In one series there were 14 beats, and these followed each other about as rapidly as I could count to myself. The other series were similar, probably no one containing more than 14 beats, quite evenly timed and with no appreciable crescendo or diminuendo. Moyla and I moved cautiously southwards towards where the sounds had come from. When we got there, there was one more series of TT sounds (the last TT and cries heard that day, although we kept the animals under observation until 12 M.), but this was to our north. Evidently the animals had moved northward. . . . .

April 13. At 6:15 the animals descended, and I saw them on the ground, going towards the west end of marigot A. Their progress was slow, and on the way the TT sound occurred four times. The first of these series I witnessed. Saw what appeared to be a large animal running rather fast through a small clearing, in the middle of which stood a huge tree whose trunk divides into, or gives off the wide, flat, solid folds (buttresses) for a short distance above the ground, as is characteristic of many of the larger tropical trees. A short distance after the first animal came a second one, also large. As he approached the tree, he gave the low-pitched panting cry several times, and then with a high-pitched scream he began pounding the folds of the tree-trunk. In this pounding, his feet were used two or three times; most of the beats were made with his hands. There were probably 8 or 9 beats in the series. When he was through, he walked off in leisurely fashion westward. The next series of TT beats I timed: 7 seconds, 11 beats. The shortest series had only 3 beats. Beats of about equal loudness (in one series) but slightly accelerated towards end.

A significant feature of sound-production in the field has already been casually mentioned. All sounds cease at the moment that presence of human observers becomes known. Apparently depending upon the degree of shock which their discovery occasioned, the animals remained silent for from 15 minutes to several hours. In one respect this behavior is adaptive, since it avoids giving the supposed enemy auditory clues as to where the chimpanzees have fled. On the other hand, failure to give a cry of alarm leaves the individuals who do not directly perceive the danger without warning.

March 17. We found the trail taken by the animals after leaving this region; it was marked by several large fresh fecal deposits. Feces were light green, very thin or watery, and contained little undigested material. The trail led in a southerly direction. Quite suddenly we saw chimpanzees in a clump of Naray trees. One large male saw me just as I saw him; he hung by his arms from a branch for a moment, regarding me, and then rapidly climbed down the tree, out of sight. (These observations were between 7:30 and 8:00 A.M.) A second, smaller, chimpanzee also quickly descended. A third animal, back to us, continued eating. Took Graflex telephoto pictures. After about ten minutes, the latter chimp turned around and saw us, quite accidentally it seemed. Walked outwards on limb of tree and was out of sight behind foliage for a moment. Reappeared for a minute and then descended the tree. Did not seem particularly alarmed. Taking all possible precautions, I crawled towards the clump of trees and caught glimpses of three more animals descending. There was no audible sounds during all this time. . . . . We approached and examined the area where the chimpanzees had been.

The more important facts regarding sound-production by free-living chimpanzees, as revealed in the present study, may be summarized as follows. Drumming and loud vocalizations occur frequently during the course of a day's activities. There may be a dozen or even more noise-making periods during one day, the duration of each period being from less than a minute to a half-hour. The sounds are often very loud, audible for a mile or more. They occur quite rarely during the hour or so preceding nightfall. There is no drumming and very little vocalization after dark.

Drumming is usually accompanied by vocalization, but the latter often occurs by itself. Drumming is probably produced mainly by the beating or striking of hollow logs and of tree buttresses. Most of the noise-making, with the exception of cries at night, seems to take place while the animals are on the ground. It is more often heard in heavily wooded regions than in open country. Almost without exception, sound-production ceases promptly when the proximity of human beings becomes known to the animals; it is not often resumed for an hour or more thereafter. Drumming shows no uniform tempo or rhythm patterns, while vocalization presents a bewildering variety of tempos, rhythms, pitch sequences and vocal qualities.

In seeking the meaning or reasons for chimpanzee sound-production, we are at once confronted with the difficult question as to whether it is primarily communicative (has the social function of influencing behavior of other individuals), or whether it is merely expressive of emotion. Without entering into a discussion of conscious intention, which, to many, is implied by the term communication, and without attempting to bring forward evidence in support of any theory, the writer formulates his own present belief as follows, hoping thereby only to present what seems the most probable state of affairs and to provide a working basis from which further observations and experimental work may proceed. Drumming and the loud vocalization thus far described are primarily affective expressions; these expressions come to be understood by members of the group in the sense that the sounds become connected or associated with concurrent or subsequent behavior towards which the animals may adapt themselves as soon as the sounds are heard; to a limited extent the individual making the sounds may observe and learn that his noise-making has in itself certain social effects, and may come to abbreviate the sound and accompanying behavior syndrome to its first element. The last step appears to be best illustrated in "bluffing behavior," so often observed in captive animals.

Certain observations made in the bush lead me to believe that there may exist, among wild chimpanzees, a crude form of language or communication which does not significantly involve

sound. The following extracts from my notes suggest three modes of communication:

1. Visual—given by gestures.
2. Tactual—given by direct contact between two animals.
3. Vibratory—Given by thuds on the ground or on trees.

It is freely admitted that the evidence indicating these three modes of communication is only suggestive, but as such it may be of value.

March 25. The sky was heavily overcast until 8 A.M. Heard chimp cries at 5:40. . . . . Approached the animals from the northwest; from some distance could see that they were in the trees near marigot N. Had my helpers disperse, so as to surround, as well as possible, the area occupied by the chimpanzees. Towards the west-northwest of the marigot there is a heavily shaded area: bushes, low trees and undergrowth, especially woody vines. Into this we penetrated a short distance (from the northwest). According to Sugeta, we were right in line with the path usually taken by chimpanzees when they leave this area. Very soon we could see, to the southeast of us, shadowy forms moving on the ground, some coming in our direction. A large female, not in period of sexual swelling, distinctly a white-face, approached, two smaller animals, not infants, back of her. She stopped about 50 feet away, and looked in our direction. She moved her head up and down, to the right and left, apparently in order better to see us. Made no sounds. She seemed to be gesticulating and pointing to the south; possibly she was only brushing a fly from her face. She retreated out of sight, the two smaller animals preceding her. She reappeared at short intervals twice more, but alone, and each time she peered searchingly in our direction, from a point some 40 to 50 feet distant. I could hear the animal walking over the dry leaves which covered the ground. I changed my position towards the southeast. There I waited some ten minutes before seeing a medium-sized animal coming towards us and then veering off sharply to the southwest and disappearing. No sound, even of locomotion. 8 A.M. Some 15 or 20 minutes later saw two chimps, one large female and one medium-sized animal, travelling very fast, to our east, in northerly direction. They left the shaded area and headed into a field of grass. . . . .

April 15. When we came to that thick jungle . . . . we saw four chimps in a fairly low, non-food tree. I think they had been resting

there. One was a young but mature female; genital region red and enormously swollen. This animal started to go to the east, in which direction the other three chimps quickly disappeared—obviously they had seen us—when she turned back and reached out an arm. At the moment I thought she was going to take something along, but could not see what, since her hand was hidden from view by foliage. Then she turned east again and disappeared as had the others. Right after her came a fifth animal, not seen before. Apparently the latter had been dozing or at least had not become aware of our presence; the female had gone back and given him some sort of signal which he promptly heeded.

April 19. Went to marigot B, to a point further south than I had been before. . . . . At 6:30 we heard the first cries. The animals were at the marigot, to our north. Approached to within about 200 feet of the animals. From 6:40 to 6:50 there was an almost continuous uproar of chimpanzee cries, mostly the excitement cry, no TT sounds. From 6:50 to 7, silence. In the meantime I approached further, alone. Entered a shallow ravine which runs into the marigot valley at an angle; i.e. the valley runs north and south, the ravine northeast to southwest. From 7 to 7:15 there was again an uproar of chimp cries, with F-P cry prominent and no TT sounds. I was probably 75 feet or so from where the animals were, but because of dense foliage I could see practically nothing. A few minutes after the cries ceased, 7:18, five animals crossed the ravine, going south. The ravine, very shallow here, is almost clear of high vegetation, about 30 feet wide. I was located a few feet towards the north of where the jungle began again; consequently was pretty well hidden from view. The five animals followed quite closely behind each other, no interval more than 5 feet. They walked rather fast and did not see me. The first four, I think, all adults. The last one, about 7 years old, was whimpering and looking backwards frequently; it seemed frightened. Face color and sex not determined for any of the five animals. For several minutes after the animals had disappeared on south side of ravine I could hear them whimpering and grunting. They evidently went up into trees, for I could see branches being moved. One animal I could see plainly in a high palm tree; another one (large, black-face) I saw feeding in a Gerenyi tree. . . . . Sounds of animals in trees to the north continued. At 7:30 a large female with remarkably narrow shoulders came from the north, going south. She emerged from the jungle at a point not more than 12 feet to my southeast and went, hesitatingly, part way across the ravine. Then, for some

reason unknown to myself, she turned around, saw me, hesitated on all fours about 10 sec. and then bounded northward, the same way she had come. Sounds of animals in trees to north continued until 7:40, those of animals in trees to the south, until 7:50. Then silence for 30 minutes. Then I heard the slow, measured tread of an animal from the north coming towards the ravine; it stopped short before emerging into view. Probably only a few yards from me; may have seen me. Then I heard what sounded like a slap on the ground; not loud, more like a thud. Almost immediately sounds of animals in trees to the south recommenced; they were evidently descending. At 8:30 I saw a female with a babe and then a young mature male cross the ravine, northward. They followed each other closely. Each hesitated for perhaps 10 seconds before crossing, looking in my direction during this interval. Then hurried across rapidly. The female gave the infant a tap on the back with her hand before crossing; the infant was hanging to the ventral side of the larger animal. The large female did not move as quickly as did the young male, for the latter, starting later, overtook the former before they were across the ravine. I waited in my place until 9 A.M. and during this half-hour heard chimps walking to north and south of me. Did the rest of the 5 animals which I had seen earlier re-cross the ravine at a point not visible to me? Possible. . . . . While engaged in this work heard chimp cries (especially F-P cry) further north; also TT sounds for first time today.

March 22. Started at 4:10 A.M. for place where Sugeta said he saw chimpanzees building nests last night. . . . . Leaving the other porters some distance away, I approached the clump of trees with two of them. As we arrived, 4:50, the first light of day was just becoming visible. We stopped under the southernmost member of the clump of trees; as we discovered later, this was about 50 feet away from the nearest nest trees. Soon we heard, but could not see, an animal jumping from one branch to another. As it got a bit lighter, 5:05, saw a chimp silhouetted in the low crotch of one of the trees, about 100 feet away. At first, so it seemed to me, the animal was eating, but this was not for long. The animal apparently saw us, and thereafter looked alternately at us and up the tree. It changed its position once, moving down still lower in the tree. Then this animal either made a start at climbing one of the two main divisions of the tree-trunk, or else it slapped the trunk; at the time I thought the latter was the case. At any rate, one of its arms reached upward and grasped or hit the wood; the animal did not, however, as-

cend. A few moments thereafter a very large chimpanzee (larger than the one thus far seen) appeared near the top of that division of the trunk. (Had slapping of tree trunk been a signal?) The latter animal looked down at us and then rapidly descended. Almost immediately, however, and before it had got to the ground, it ascended the other bifurcation of the tree-trunk. A moment later it was again descending, this time followed by two medium-sized animals. That made four animals which had come down from this tree. Changing my position slightly, I saw three chimps, two of them medium-sized, one large, in a tree a little to the southeast. They descended almost as soon as I saw them. . . . . There were no audible sounds during all this time. By 5:35 it was fairly light (although sun was not yet visible) and by that time all the chimpanzees were gone. We went closer, for a detailed examination of the territory. . . . . Found nine fecal deposits in all, but some of these were small and close to each other. . . . . There were 16 nests, 10 of which the natives claimed were used that night.

April 20. At 6:35 heard chimpanzee cries coming from near top of hill, to our northwest. We climbed this hill and suddenly saw, to our north, two animals rapidly descending a Seekunyeh tree, 6:55. Probably saw us. No sounds. One animal, a black-face, could be seen still in the tree, sitting rather passively in a low crotch. Other animals apparently also in the tree, eating. We were about 150 feet away. About 7 minutes waiting and watching, and then could hear chimps on ground, approaching us. When they were about 25 feet away—the grass here was thick and high—they stopped and one of them stood upright to look around. The two chimpanzees immediately ran away as fast as they could go, making much noise in crashing through the grass but not vocalizing. . . . . Four minutes later there were short excitement cries about 300 feet to the west. I followed the sounds, alone. Before I had gone very far, I could hear animals walking on the ground; decided that they were following the trail, to my south, which leads down the hill and into an almost impenetrable forest. I ran southward as fast as I could until I was within 40 feet of this trail, then crouched. Very quickly I saw 6 animals descending the hill along this trail. They were walking single file, close behind each other. One had infant clinging to ventral side. There were no cries. After they had passed me, I remained quietly where I was. About 4 minutes later another animal came, but stopped on the trail just about opposite to where I was. It was an adult female, black-face. Right near where she stopped there was a Naray tree con-

taining one very large, new nest.   The nearest other new nests were about 150 feet up the hill.   Between 7:30 and 7:40 this animal stayed at about the same spot and 8 times gave the "cry of exasperation," a panting cry tinged with fear, impatience and anger.   Each time that it gave this cry it beat on the ground or on a tree one to three times.   Once there was a scream of high-pitched wailing sound; at the time I thought of it as a "cry of despair."   In general the animal seemed nervous, irritable.   Once when she gave the cry of exasperation, she simultaneously grasped a low-hanging branch and pulled this up and down violently a number of times. At 7:41 I approached closer; the female moved down the trail a little further.   Almost certain she must have seen me, but her attention did not seem to be particularly directed towards me.   Silence until 8 A.M., then another "exasperation cry". . . . . At 8:15 another such cry, still further down the hill; I had lost sight of animal by this time.   At 8:30 one more cry, and then no more.   I followed the trail until it entered the thicket below, but saw no more of the chimpanzees.   During all the time that I had the single adult female under observation there were no sounds from the first 6 animals which had passed down the trail. The unusual behavior of the female has several possible interpretations. It seems probable that she got separated from the rest and was trying to find her way to the group by soliciting a rescuer or signals.   For one reason or another, the rest of the group did not respond, at least vocally, to her cries and beating of the ground and trees.   This beating, incidentally, did not give anything similar to typical tom-tom sounds; it was a dull, thud-like sound, barely audible 50 feet away.

The great difference between the sounds of drumming (probably beating of hollow logs and tree buttresses) and those produced by striking the ground or thick, solid tree trunks, probably is largely attributable to the nature of the inanimate instrument, although some difference may exist in manner of striking the blows. Various observations (page 14) suggest that the chimpanzee is not overly sensitive to sounds.   Ground-stamping produces little sound, but may produce vibrations perceptible for reasonable distances and not readily identifiable, to man or other animals, as coming from a chimpanzee.   Drumming, to the contrary, is very loud and definitely discloses presence of the apes.   Vocalization and drumming end promptly when proximity of supposed danger becomes known, whereas ground-stamping or striking of solid

wood apparently may occur under such conditions. It is these considerations which have led to my tentative hypothesis that chimpanzees may use vibratory stimuli in communication. In the case of the last illustration cited, however, it must be admitted that the ground-stamping may have signified nothing beyond convenient emotional outlet by a greatly disturbed animal. Laboratory experiments on sensitivity of chimpanzee to vibratory stimuli, it may be expected, will show whether the above hypothesis is at all tenable.

8. CONCLUSIONS

In the foregoing pages have been presented what appear to be the most significant results of two and one-half months' observational work in the bush of French Guinea. These results, above separated and treated under several more or less logically determined categories, may now be considered briefly in their broader aspects and relationships. How well is the native habitat of chimpanzees suited to their needs or requirements; how does the chimpanzee adapt himself to his environment, and how does he adapt the environment to meet his needs?[10] When these three questions have been completely answered, the curiosity of the student of animal behavior concerning non-captive chimpanzees will be satisfied to a considerable extent. But not entirely. In addition to the above questions, which imply no unusual assumptions or suppositions, another query may be posited, one which admits the possibility, not often denied, but seldom emphasized, that an organism may be capable of surviving in a much more difficult environment than that in which he actually lives. To what extent does the wild chimpanzee exploit his behavioral potentialities; or, putting it another way, what is the achievement quotient of the chimpanzee in his native habitat? Without presuming to give even an outline answer to any one of our four questions, we may attempt to fulfill the purpose of this section, as stated above, by applying them to our observations.

[10] These questions are, of course, inseparable, their answers mutually dependent. Organism and environment are interacting systems; the environment cannot be changed without changing also the organism.

The physical environment involves principally food and water supplies, provisions for shelter, and conditions of locomotion. Inasmuch as chimpanzees in the bush appear to be fairly healthy, we may assume that the factors of climate, food and water are reasonably well suited to these apes. Presence of occasional respiratory troubles, as indicated by my observations, and the large proportion of lean, possibly undernourished, individuals, do suggest that adaptation between environment and animal is not ideally perfect. Furthermore, the fact that chimpanzees do survive pretty well when transferred to a temperate climate, although with man-provided shelter, food, and temperature, might seem to indicate that the specific physical environment provided by their native habitat is not essential to chimpanzees. Indeed, this primate obviously finds the sun of French Guinea uncomfortably hot at times, and takes great pains to avoid direct exposure to its mid-day radiation. Nevertheless, it is probably true that chimpanzees find the climate of their native environment relatively satisfactory. Food, apparently to the taste of the chimpanzee, is provided in abundance by his surroundings. He has only to climb trees and pick it off. Water, of which he needs little, is usually easily available. Trees in which he builds nests at night and in whose shade he spends most of the day, provide shelter and escape from certain enemies. The high bush grass also helps him to elude those who would harm him. In general we may say, then, that the particular habitat of chimpanzees, which we have studied, provides the animals adequately with all external essentials of life.

The necessary adaptation on the part of the apes to such surroundings is given largely in their motor and sensory equipment. Excellent vision and muscular development enable them to avoid most of the dangers of the bush and to find and attain their food. When fighting becomes necessary they can rely on their tremendous strength and good teeth to protect them. Usually, however, their timidity, served by keen eye-sight, allows them to avoid serious encounters. Competition for food, at least with animals who would resist the chimpanzee, seems to be largely absent. Nest-building is certainly a performance requiring a

certain degree of skill, but not any more than is manifested by lower animals, birds for instance. In general there appears to be small need for exercise of the highly developed nervous organization which, as laboratory experiments show (see, for instance, Bingham's work, 1, 2), these animals possess. The material requirements of chimpanzees, according to my observations, are almost as simple as those of lower primates inhabiting the same region, and certainly the ape is at least the equal of his fellow primates in muscular and sensory equipment necessary for the attainment of those requirements. It is hardly surprising that when wants can be satisfied by what we may call primitive behavior, complicated and therefore difficult behavior mechanisms will not be brought into play, even if potentially present. Man himself works no harder, and especially thinks no more, than he must in order to get what he wants. But man wants a great deal, the chimpanzee very little. When we bring the ape into our laboratories, we make the conditions for attainment of needs more difficult; more objectively stated, we place obstructions between a motivated animal and the external condition (incentive or lure) which will "satisfy" the motive. Primitive or simple behavior no longer being adequate to meet the situation, the animal is forced to use the upper ranges of his intelligence—whatever that may be. If living conditions in the bush were to become more difficult, or if chimpanzees suddenly were to be deprived of some of their motor or sensory capacities, I think that we might expect to find these animals exercising their ingenuity much more than they now do. Perhaps the chimpanzee actually exploits his nervous organization in the bush far more than my incomplete observations would indicate. As a matter of fact, I am omitting from specific consideration here certain observations of my own which, although illustrating complex behavior in wild apes, do not significantly affect the viewpoint being presented. Future field work may well be expected to reveal many instances of so-called intelligent behavior. But it is nevertheless my opinion that field work will not disclose the upper limits of chimpanzee intelligence; that the ultimate capacities of these apes for complex behavior will be found and measured in our laboratory experimental situations.

Chimpanzees, at least relatively young individuals, are energetic creatures. My observations would seem to indicate that only a small proportion of their energies are expended in activities relating specifically to "making a living." The remaining excess of time and energy, muscular and nervous, flows mainly into the channels of play, fighting and perhaps reproductive activity. How much time is devoted to implicit behavior is, of course, hard to say; at any rate, few outward manifestations or consequences of it were found. In play and in various forms of social behavior, I believe, we are most likely to find manifestations of the intelligence of wild chimpanzees. But, as was pointed out earlier (pp. 78–82), the play of these apes in their native surroundings probably turns largely to gross activities, involving the use more of brawn than of brain, whereas under conditions of captivity movement is restricted and more subtle forms of recreation must be sought. Judging from the stupendous amount of vocalization and drumming heard in the bush, we may surmise that the chimpanzee is extremely emotional. The probability of that hypothesis depends importantly on the acceptance of the interpretation of these sound-productions as being almost exclusively affective expressions. Assuming such acceptance, I would suggest that the emotional life of these apes is developed (intensively more than extensively) to a much greater extent than is the so-called intelligent or intellectual aspect of their activity, and that in their emotions, or in behavior which we call emotional, these animals find outlet for much of the excess energy left over after the simple essentials of life have been procured. Inasmuch as external conditions of life are relatively favorable (food, water and shelter easily available, enemies not often troublesome), and since expressions of emotion, or emotional behavior, seem to be connected with domestic affairs rather than with matters of the environment, it appears plausible that affective behavior is connected mainly with social relations within the group.

Although my relatively short stay in the bush of French Guinea resulted in the accumulation of more factual material than I had dared to anticipate, the story of the chimpanzee in his native habitat has only been started. So far it is an ill-balanced, uneven

narrative; some few aspects have been covered relatively thoroughly, while many others have been barely touched. Much interesting and profitable work lies ahead. With certain notable exceptions, our earlier notions on the subject have come from incidental observations of naturalists or travelers primarily interested in other things. Some of these notions the present study has confirmed, a few others it has disputed; some bits of information it has added. The major share of the task remains to be done. That it needs to be done by thoroughly trained observers is obvious. That it is very much worthwhile doing, I think is equally clear.

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PASTORIA

FIG. 1. General view of buildings and grounds
FIG. 2. The main building

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PASTORIA

FIG. 3. One section of chimpanzee quarters
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Fig. 5. Locust invasion
Fig. 6. A cloud of locusts approaching Pastoria

FIG. 7. Young chimpanzees at Pastoria
FIG. 8. Supervised play at Pastoria. There are eleven chimpanzees in and
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Figs. 9, 10 AND 11.   Recreation time at Pastoria

FIELD HEADQUARTERS

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FIG. 13. At Kambi

NATIVE ASSISTANTS

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mass
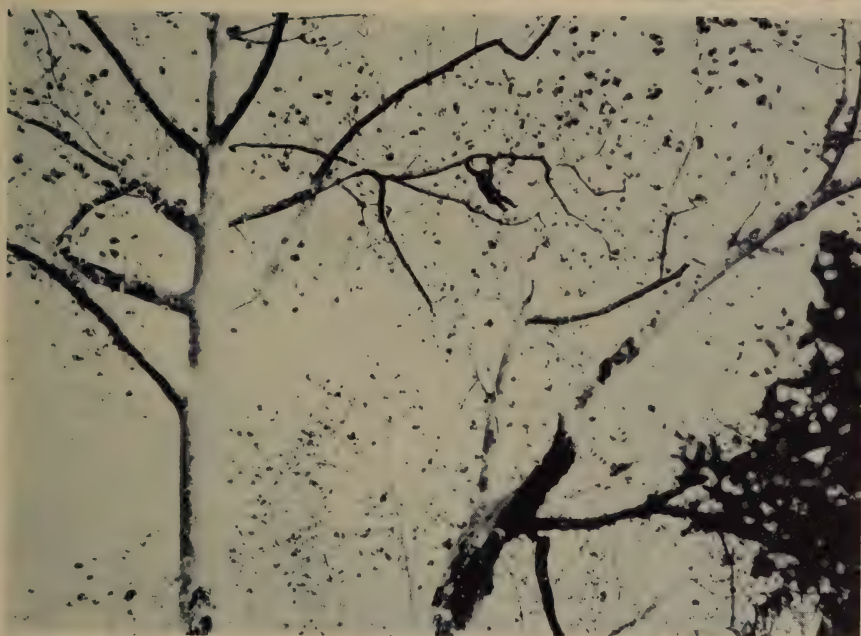
FIGS. 24 AND 25. The lone female chimpanzee observed March 20th
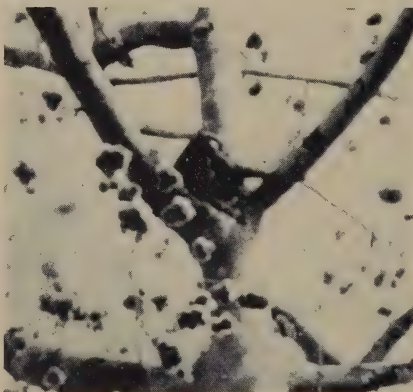
FIGS. 26, 27, 28, 29, 30 AND 31. Chimpanzee in motion and at rest. This is the same animal pictured in figures 24 and 25

Figs. 32 and 33. Typical chimpanzee nests

Fig. 34. An atypical nest, supported from beneath by a large horizontal branch

Fig. 35. Two Naray trees containing ten nests

Fig. 36. Looking down on a chimpanzee nest which had been occupied the previous night

Fig. 37. Showing how branches are interwoven to produce the foundation of a nest

FIGS. 38 AND 39. Upper.   Kambi, one day after capture
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A FURTHER STUDY OF THE DELAYED REACTION IN THE ALBINO RAT

BY

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A FURTHER STUDY OF THE DELAYED REACTION IN THE ALBINO RAT

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I. INTRODUCTION

Natural history is replete with descriptions of situations in which animals, in their natural habitat, apparently are reacting to a determining stimulus which is not present at the moment in which the response is made. A typical example of such behavior, provided we ignore possible olfactory stimulation, is provided by the grey squirrel in the following type of behavior: The animal may frequently be observed burying nuts and other bits of food in the ground in the environs of its home tree. At some later time the animal may, furthermore, be observed returning and, without any apparent hesitation, going to the spot where the food was buried and recovering it. It has generally been supposed that it was guided during this behavior by the olfactory sense but the observations of Frost (5), Klugh (14), Dice (4) and others indicate that it may be necessary to find some other mechanism whereby to account for this behavior for these observers report observations, which, while they do not entirely eliminate the possibility of olfactory stimulation, yet, make such a possibility appear rather remote.[1] Nor is this type of behavior

[1] Frost observed a grey squirrel detecting and recovering buried nuts and assembling these nuts at a single protected spot after a snow fall of about 2 inches.

limited to the squirrel. It may, if we again neglect possible olfactory stimulation, be seen in the carnivorous animal stalking its prey in the dense undergrowth of the forest. Domesticated animals also exhibit the same type of behavior under various conditions.

Instances of behavior such as the foregoing have been noted by psychologists and others and various explanations have been proposed. These range all the way from explanations attributing ideas and mental images of the various objects which served as stimuli, to explanations which deny to the animal under such conditions all behavior except such as is directly due to direct stimulation and its immediately accompanying response.

In order to make an analysis of the type of mammalian behavior which has been mentioned above, under the more controllable conditions of a laboratory experiment the delayed reaction experiment was instituted. Here associations are set up between some stimulus, for example, a light, a sound, certain movements of the experimenter, etc., and certain movements on the part of the animal which produce some form of reward for the animal. Having established this association, a period of delay is introduced between the moment when the stimulus is presented and the moment when the animal can make the particular series of movements leading to food or some other form of reward. If the animal can still react correctly after a period of delay in which

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Eight nuts were recovered while the observer watched. Only one time did the animal dig without apparent results. In five of the eight times the squirrel first mounted a post and apparently surveyed the untracked snow then went directly to the place where the nut was buried. Klugh reports observations on a squirrel which had buried nine nuts in the snow on his balcony. During the night there was another snow fall and the new snow covered the former snow by an additional foot. The next morning the squirrel returned and without hesitation dug up the nine nuts. Dice, in turn, reports an experiment which he performed with a fox squirrel which was in the habit of coming to a porch of his home and securing black walnuts kept there in a box. Finally, the nuts were removed and the box filled with dry sand. Some black walnuts were completely buried in the sand, others were partially buried while others were exposed to view. Even though the squirrel returned frequently to the box it never found any of the nuts save those which were exposed to view. It frequently dug in the sand in the box but found none of the hidden nuts. If only the tip of a nut was exposed it was found sooner or later but never when the entire nut was completely covered.

the determining stimulus is absent, it must have some mechanism that functions for the stimulus in initiating and guiding the correct response.

In any study of the delayed response two principal questions arise. The first of these questions deals with the period of time over which the animal can delay after the determining stimulus has been withdrawn and still make the correct response. The second question is concerned with the mechanism by means of which the animal is able to bridge the delay and yet react correctly after the period of delay. This latter question must be answered on the basis of behavior cues which the animal may demonstrate during the time that it is in the problem situation or on the basis of controls which the experimenter may introduce into that situation, or perchance by a combination of these two conditions.

Numerous experiments have been performed on animals selected from the different species of the animal kingdom from the chicken to the human child of eight. On the basis of those studies in which the animal was capable of making the correct response after the delay, it appears that the delay period is bridged either (1) by maintaining overt bodily attitudes and then after the delay reacting on the basis of those overt bodily attitudes, or (2) by means of some intra-organic cue which may be maintained during the delay or be reinstated after the period of delay. Several studies (6), (21), have indicated that rats, dogs and cats were able to react correctly after delay provided the first of the above methods was used in bridging the delay whereas raccoons, apes, and children could react correctly when these overt bodily attitudes were not maintained and consequently must have been reacting on the basis of some intra-organic cue. Some more recent experiments on dogs (19) and cats (1), (3) indicate, though not conclusively, that these animals too, can delay without maintaining bodily orientation.

Since Hunter's experiment no one had utilized the albino rat as subject in the delayed reaction experiment and yet this is probably the most favorable animal to use in studying this problem provided it can be demonstrated to possess the ability to react

on the basis of non-overt bodily attitudes. While Hunter's experiment shows conclusively that in the particular situation used in his experiment the animal maintained gross bodily orientation yet by using a different situation it might be possible to discover that, when circumstances demanded, the rat too could react without bodily orientation.[2] This belief is given additional credibility when we consider the squirrel mentioned in the early part of this section. Here is an animal which is apparently reacting to an absent stimulus, if we neglect the not entirely eliminated olfactory sense, after a long period of time without orientation in the meanwhile and yet which belongs to the same general class of animals as does the rat, namely, the rodents. If then the squirrel is able to react on the basis of some intra-organic cue we should expect that the rat, which has approximately the same type of nervous system as does the squirrel, should likewise be able to react on the basis of intra-organic cues provided the situation can be found to bring out such behavior. Reaction by gross bodily orientation is probably easier for the animal than the building up of intra-organic cues but what will the animal do when it gets into a situation in which it cannot react on the basis of gross bodily orientation?

Furthermore, the only type of stimulus that had been employed in experiments on delayed response in the rat were visual stimuli. Yet it is a well established fact that these animals make little use of vision in most situations. It might then, be possible that the ability of the animal to delay has been tested by means of one of its least used sense avenues. It is quite possible that were some other sense used, for example, one which the animal ordinarily uses in its more natural environment, that not only would the period over which it could delay be lengthened but it might be found capable of responding correctly after delay without having maintained gross bodily orientation.

In view of the foregoing considerations, it was decided to make

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[2] Since the formulation of and experimental work on this problem was undertaken, there have appeared in the literature two studies on rats (26, 27) in which by the use of different situations evidence was found that the animals reacted without maintaining bodily orientation.

an attempt at a further analysis of the behavior of the albino rat in a situation involving a delayed response. The only type of situation involving the delayed response, in which this animal had been tested was that used by Hunter (6). In that situation a stimulus is presented to the animal and then after a period of confinement in a relatively small space, the animal is released in order to react to the stimulus which has been removed in the meanwhile. It is also possible to arrange a situation which will require response to an absent stimulus but instead of keeping the animal confined during that period of delay, will keep the animal in motion between the giving of the stimulus and the time when the final reaction to that stimulus is required. A situation such as this is probably more nearly the type of situation which the animal would meet in its natural habitat than is that mentioned as the first method above. Accordingly this is the situation which was used in the experiments reported in this study.

In addition to the study of the delayed response in a situation different from that previously used with the albino rat, we attempted to study the effect on the delayed reaction of stimulation through various senses. These problems were studied with reference to their bearing on the aforementioned questions which arise in any study of the delayed response, namely, (1) the period of time over which the animal can delay after the determining stimulus has been withdrawn and still make the correct response; and (2) the mechanism whereby the animal makes the correct response.

II. CRITICAL REVIEW OF PREVIOUS WORK

The first attempt to study the delayed response under the controlled conditions of a laboratory situation was made at the suggestion of, and under the direction of Harvey A. Carr at the University of Chicago. The earliest actual work on this problem, suggested by Professor Carr, was done by a graduate student, W. R. Hough, whose work was followed by work on the same problem by H. S. Reed. Neither of these two students carried the work to completion and as a result their work served rather as preliminary to the later work of Walter S. Hunter

which was done during the years of 1910 to 1912. Since Professor Hunter's experiments with rats, dogs, raccoons and children, a number of researches have been made on the delayed response utilizing various species of animals, i.e., cats, dogs, monkeys, apes, and children, as subjects.

A. Methods and apparatus used in studying the delayed reaction

Hunter (7) has pointed out the essential features which any apparatus and method must embody in order to study the delayed response. These as noted by him are:

1. It must be adapted to the size of an animal or subject and its mode of response.
2. It must provide a means for presenting a stimulus in one of several places.
3. These stimulus places must be equally accessible to the response.
4. The stimulus and method should be such as to present no differential cues to the subject during the intervals of delay.

These features have been embodied in the experiments which have been done on this problem or at least an attempt has been made on the part of the experimenter to do so in devising his apparatus and method and I believe that in the main they have succeeded except it might be that in some cases the fourth of these features has not been sufficiently guarded and if so the experiment was no delayed reaction experiment. In general the methods which have been used may be divided into two general classes, namely:

1. The "indirect method," which was the first to be used, and
2. The "direct method."

In the following pages of this section we shall attempt to describe first these two general methods of studying the delayed response and some of the variations employed by the different experimenters using the method and second, the results obtained by these experimenters under the various conditions which they set in their experiments.

The principle involved in the studies using the indirect method is that of establishing, by trial and error learning, an association

between a stimulus, i.e., a light in a certain position, or a sound,
and a series of movements on the part of the animal, which lead to
food.   As soon as the animal has learned the relationship between
the stimulus serving as the cue and the food the actual experiment
involving the delayed reaction is instituted.   Instead of having
the animal make the necessary series of movements while the
stimulus calling them forth is still present, the stimulus is given
and then, only after a period of time, is the animal permitted to
make the final reaction necessary to obtain the food.   This was
the method first used by Hunter (6) in his famous experiments
and later by Ulrich (18), Yarborough (21), Walton (19) and
Honzik (27).

In order to secure a situation adapted to the above method,
Hunter (6) used a box composed of five chief compartments namely,
a release box, a larger compartment into which the animal was
released from the release box, and three smaller compartments
each containing a light which, when lighted, served as the stimu-
lus.   Small doors directly in front of the lights connected these
light boxes with the larger compartment.   The light boxes, on
the other hand, were so arranged that the animal after entering
them left by means of a second opening and was directed back
to the entrance of the release box where, when the proper choice
had been made, it obtained food.   (This was the arrangement
used by Hunter with his raccoons, dogs and some of the rats.
With the rest of the rats, food was given at the exit doors of the
lighted boxes.   Hunter does not state explicitly which animals
composed the latter or former groups of rats unless it is implied
that the ones trained without punishment were fed at the exit
doors of the light boxes.   He does state that the one of the two
boxes used with rats was not wired for punishment and that in
this box the animals were fed at the exit doors.   This may imply
that the group trained without punishment were all trained on
this box but it does not necessarily do so.   If they were, then
the animal with the longest delay was trained with food given at
the exit door of the lighted box.)   Ulrich (18) in his study on the
integration of movements in learning in the rat studied the de-
layed response in connection with "posturing" and used the same

type of apparatus except that it was larger than Hunter's and instead of rewarding the animal in the vicinity of the release box the animal was rewarded with food in a second compartment attached to the light or stimulus box. Furthermore, instead of supplying his stimuli by means of 8 c. p. bulbs as Hunter had, he used 50 Watt bulbs. Both of these features, the offering of reward in the compartment attached to the stimulus box and this larger light, would increase the difficulty of controlling these features. It is conceivable that in this apparatus immediate stimulation may have been present. Yarborough's (21) apparatus was a duplication of Hunter's apparatus except that it was adapted to the increased size of his animals (cats). His method also was identical with that of Hunter's. In addition to using light as the cue, he also used sound supplied by means of small buzzers suspended in the stimulus boxes by means of coiled wire, attached to the ceiling.

Walton (19) used an apparatus which while identical in principle with that of Hunter's differed somewhat in arrangement. The animal was confined during the period of delay in a release box adapted to the size of the animal, a dog in this case. Directly opposite and about 12 feet from the release box were three stalls holding the stimulus lights. These three stalls were on the same ground level. In some of the tests, a fourth stall was added which was not on the same ground level as the others but was located above the others and was reached by means of a plank placed at a 30 degree incline. Reward was given in the correct stall whenever the animal made the correct choice. Varying conditions were investigated in the experiment. These were for the purpose of studying the difference in reaction when the release box faced the stalls containing the stimulus; when the release box faced the food compartments during the giving of the stimulus but during the delay period was turned at an angle of 90 degrees; and when diverting stimuli were applied during the delay both when the release box faced the stalls, and when it was turned at the 90 degree angle. The diverting stimuli consisted of (1) the sight of the experimenter walking around in front of the release box during the period of delay; (2) the experimenter

calling the dog; and (3) the operator whistling to the dog and hanging a piece of meat at the front of the release box.

Honzik's (27) apparatus for studying delayed reaction in the rat also utilized the general principle of Hunter's set-up but differed from it in that the animal, by running up to the stimulus, made a more definite reaction to it.   Advantages of this arrangement were that the experimenter could determine that the animal had received the stimulus and that it might serve to reinforce the stimulus.   The apparatus consisted of a release box; a large discrimination chamber at one end of which were three doorways covered by stimulus curtains, two black and one white; and an elevated path leading to food boxes.   Directly in front of the stimulus curtains was a large screen which was lowered during the delay period.   In the delayed reaction tests the animal, released into the discrimination chamber, ran toward the white curtain.   Just before it reached the curtain, the screen was lowered and the animal was confined in the discrimination chamber for the period of delay, at the end of which the screen was raised and the animal was confronted with three black instead of one white and two black curtains.   Its problem was to pass through the door which originally contained the white curtain.   Punishment could be administered when the animal attempted to pass through the wrong door.

The principle involved and the apparatus used in the direct method differ somewhat from those employed in the indirect method.   The principle employed in this method is that of utilizing an association, between some stimulus and reward, which has been previously formed, or, at least, one which is so intimately connected with the animal's past experience or native constitution that little or no training is required to set it up.   After the presence of the association has been determined, a period of delay is introduced between the giving of the stimulus and the final reaction.   In the use of this method various forms of apparatus have been employed.

The direct method was first used in investigating the delayed response by Hunter (7) in the study in which his daughter, Thayer, was used as subject.   In this experiment the small child

was seated in front of three small boxes. The experimenter permitted the child to see him hide a toy or other stimulus object in one of the three boxes. During the following period of delay the child was disoriented and then at its conclusion was permitted to take the toy or other stimulus object from the box, provided a correct choice of box had been made. Hunter points out in the study that here an association was used which did not have to be learned at the time of the experiment. Yerkes (23) in studying the gorilla, Congo, studied delayed response by the same method, except that instead of only three boxes, five were used. This was the multiple choice apparatus designed by Bingham. Directly outside of the grill forming the front of the gorilla's cage was placed the board holding the five choice boxes, the board being so arranged that the ends slid in ways on the platform. This enabled the experimenter to pull the board out of the reach of the gorilla when the stimulus was to be placed into the box as well as during the delay period. At the end of the period of delay the board was pushed to within the reach of the animal. During the delayed reaction experiment, the animal was permitted to see the experimenter bait the proper box. The period of delay then intervened and, at its conclusion the board containing the boxes was pushed within the reach of the animal which was permitted to select one of the boxes. If the choice was correct the animal obtained food from the box, if incorrect it found the box locked. Tinklepaugh (17) in his study on representative factors in monkeys used a method and apparatus similar to that of the two preceding investigators. It differed, however, in that the animal was placed on a chair about eight feet distant from the food containers of which there were only two. The animal was again permitted to see the experimenter bait the food container. A board used as a screen was then placed between the animal and the food containers during the delay period. At the end of the delay the animal was given the signal to leave the chair to go and get the food. Various other situations were used in which the animal was disorientated and distracted during the period of delay.

A somewhat different arrangement of apparatus is found in

the experiments of Yerkes (23) on the further study of delayed
reaction in the gorilla, of Yerkes and Yerkes (25) on their study
of Chimpanzees and of Adams (1) in his study on cats. In these
three studies the following procedure and arrangement of appara-
tus was employed. The animal was confined in the center of the
experimental situation. In the two former studies, the gorilla
and the chimpanzees were tied to a mooring post whereas in the
latter study the animal was confined under a wire basket in the
center of the room. In four different directions from this place
of confinement were placed small boxes which were identical ex-
cept for certain factors such as color, etc. The procedure con-
sisted of allowing the animal to see the food being placed in
the box. Then during the period of delay the behavior of the
animal was either observed by the observer from another room,
if the animal remained in the situation, or the animal during the
longer delays was removed from the room entirely and either set
to other tasks or taken back to its sleeping compartment. After
the period of delay the animal, if it had been removed, was re-
turned to the mooring post or cage (in the case of the cats) and
after a brief interval released. If the animal went to the proper
food box and opened it, food was obtained, otherwise no reward
was obtained.

Yerkes (24) in his further study of the gorilla, Congo, due to
the limitation of space which prevented using the above method
and further intending to avoid the factor of location in which
the food container was baited, devised and used the following
apparatus and method. A turn-table 60 inches in diameter was
mounted on a platform directly in front of the grill of the go-
rilla's cage and 24 inches from it. On this turn-table were placed
six tin kitchen canisters. The canisters differed in size whereas
the sets were distinguished by means of different colors. This
made it possible to vary either size or color. The procedure con-
sisted of baiting the can directly in front of the grill and covering
it. The table was then rotated until the food container was in
some other position than that in which the animal had seen it
baited. After an interval of delay the gorilla was permitted to
reach through the grill and turn the table so the proper canister

came into position directly in front of and at the nearest point to the grill. When it had been turned into that position, the experimenter gave the animal the food provided the proper canister had been selected, otherwise the animal received no food.

Maier (26) attempted the use of the direct method in studying the delayed reaction in the rat. His set-up consisted of three ring stands placed on the floor of a room which had previously been thoroughly explored by his animals. From the floor the animal could climb to the top of a ring stand by means of a wire ladder and thence, if the correct ring stand had been chosen, could pass to the food table via an elevated path. His method consisted in placing the rat at the bottom of the selected ring stand and then allowing it to climb to the top and cross the elevated path to food. Two similar trials followed immediately and then a period of delay was introduced. After the delay period the rat was placed on the food table but in such a position that it was unable to get food except by descending a ring stand adjacent to the table, crossing the intervening floor space to the ring stand used in the three preceding stimulus runs and then proceeding as in those runs.

A somewhat different type of situation and yet one which involved the direct method was that employed in the food burying experiments of Kohler (12), Yerkes (23), Yerkes and Yerkes (25), and Revesz (16). In all of these experiments the animal, from its place of confinement, was permitted to see the food buried in a homogeneous plot of ground. After the food had been buried the animal was removed. Immediately after the removal of the animal, if the delay was to be one of considerable length, precautions were taken to prevent odor of the food from permeating the ground. After the period of delay the animal was returned and released in order to see if it could still find the location of the food and recover it.

A more complicated situation involving the direct method was the one used by Kohts as reported by Yerkes and Petrunkevitch (13). This might be designated as the method of "choice by sample." She first taught her young chimpanzee, Iona, to match

colors, etc. She then handed the animal some material, of a certain color, for example, and the animal was required to pick from an array of other colors on the table the one corresponding to the color which he held in his hand. Having learned to do this the animal was given material of a certain color which after a few seconds was again taken away. A period of delay then intervened before the animal was permitted to select the material corresponding to the sample.

Buytendijk (2) in attempting to test out the delayed reaction by a more natural method than that employed in the researches of Hunter reports that he accomplished this by throwing a piece of food to the animal and then while the animal was watching threw out three additional pieces. The animal ate the first and then after various periods consumed in eating, etc. went directly to the others in turn and ate them.

Other studies which involved the direct method to a greater or lesser extent were those of Cowan (3) and of Nellmann and Trendelenburg (15). Cowan utilized an ordinary living room with doors at opposite sides of the room connecting with a kitchen. The subject, a cat, was confined either on the sofa or in the center of the room. The experimenter appeared at one of the other of the doors holding in her hand a dish containing a piece of food. As soon as the cat had apparently seen her, she retired to the kitchen and placed the food on the floor under the stove. The animal was then released and observed, when she reached the kitchen, by the experimenter through a small window from the porch. If the cat had come through the same door at which the experimenter had last appeared she obtained the food, otherwise the food was taken away from her.

Nellmann and Trendelenburg (15) employed a small problem box, the door of which was hinged at one side and opened at the other side. The animal, a monkey, was shown the food being placed in the box. On being released he was required to go to the box and open it in order to obtain the food. The problem was now complicated by turning the box at various angles so that the animal had to approach the box from various directions to open it. Having learned to go always to the side of the box on which it

opened, periods of delay were instituted. The animal now had to remember during the period of delay whether the box had been turned or not from its original position and if so in which direction.

It must be understood, of course, that when we divide the foregoing methods of investigating the delayed response into "indirect" and "direct" methods we make such a division on a relative and not on an absolute basis. These two methods are not mutually exclusive but are only relatively different. Both involve the use of associations which must be established at sometime or other during the animal's experience. In this respect the methods are the same but they are relatively different in that the indirect method makes use of an association which must be built up "de novo" or at least nearly from that point whereas in the direct method an association is selected which, at least, is partly learned before the experimenter undertakes training the animal. In the course of a relatively few trials the experimenter perfects the particular association to a point where it can be used satisfactorily in the investigation.

Then too, the two methods differ in the particular type of association used, that is, in the indirect method the association which is set up is usually one such as between a light, a sound or some similar stimulus and food. This is a type of association which the animal has not experienced in the past. In the case of the direct method, the association is usually between seeing certain movements on the part of the experimenter and food or perchance between the seeing of food and certain movements on the part of the animal. As far as the present writer is aware the exact association or associations involved have never been determined. In the situations employed there is always a possibility of several associations that might be involved.

One of the criticisms which has been put forward against the indirect method is that first pointed out, I believe, by Buytendijk (2), namely, that an attempt is made to use an association which has no biological significance for the animal. Köhler (11) has made about the same criticism when he states that in using a discrimination method of investigating memory we are presenting the animal (apes) with a situation which presents no sensible

relationship to the animal, i.e., it cannot give evidence of insight into the problem but must learn the problem by trial and error just as the rat or any other of the mammals. Now the question arises as to why the primates cannot see a sensible relationship in this particular problem. Is it due to the fact that lights have no biological significance for the animal or is it rather that here we reduce the ape to exactly the same position as the rat and compel it to form an association from its very beginning rather than by taking a situation which has previously been experienced, at least in part, and has already acquired a certain "meaning" for the animal? This may merely be evidence pointing to what may be a fact, namely, that the actual learning ability of the ape is not so much higher than that of the rat when they are learning equally new situations.

It appears to the writer that the difference between the two methods are differences similar to those found between non-sense and meaningful material as employed in the investigation of memory in the human being. Here, too, we find differences in the rate at which the necessary associations are set up and possibly in the period of time over which the material can be recalled or reinstated. Both have been used in the investigation of memory but when we want to study the memory process under the most completely controlled conditions non-sense material is used rather than meaningful material.

Some experimenters attempting to use a direct method have utilized what have been called food burying experiments. With this method delays of much greater duration have been found for primates than with any other methods. Yerkes (23, p. 98) points out that this is a "crude test or method of sounding for memory" since it does not exhibit anything peculiar to apes but that there is evidence that other animals exhibit it in a very proficient manner.

B. Experimental results obtained in previous studies

As was previously pointed out, two main problems arise in the study of the delayed response experiment. These are: (1) How long can the animal delay after the stimulus has been re-

moved and still react correctly, and (2), what is the mechanism by which the animal reacts after the delay to a stimulus which now is absent? Of these two problems, of course, the second is considerably the more important.

1. Length of delay obtained with different species of animals. The albino rat is probably the lowest species in the biological scale which has been found capable of reacting in the delayed reaction problem. Revesz (16) attempted the direct method using chickens as subjects but was unable to secure a delay long enough even to bury the grains and then have the chicken peck or scratch for them. The animal refused to react to the grains as soon as they were out of sight.

Two experimenters have previously worked with the albino rat, Hunter (6) and Ulrich (18). Both of these experimenters found that the animal was able to react to the absent stimuli, but the period of delay found by Hunter did not exceed 10 seconds when three boxes were used, and that length was found in only one case. He used fourteen rats. Four of these were able to react after a period longer than one second, the others were one second or less. When, however, he employed only two of the three stimulus boxes he found that the period of delay for three of the five rats so tested increased from one to five seconds. Hunter's results were computed on the basis of 25 trials giving ten trials per day. Ulrich tested eleven rats and found periods of delay up to 50 seconds in the case of one animal, but in this as in the case of a number of animals, the percentage of correct choices was only slightly above what one would expect on the basis of pure chance with an infinite number of trials. Furthermore, some question might be raised as to controls used in the experiment.

Two more recent experimenters Maier (26) and Honzik (27) have also worked with the albino rat. The former utilizing a direct method reports delays up to 24 hours, while the latter experimenter reports delays ranging from 7 seconds to 45 seconds with some evidence of delays up to 3 minutes.

Yarborough (21), Cowan (3) and Adams (1) have used the cat in studying the delayed reaction. Yarborough as indicated

above used the indirect method, Cowan a modified direct method and Adams used the direct method as previously used by Yerkes in his study of the gorilla. Yarborough found that when three choice boxes were used the cat could delay for periods of from 2 to 4 seconds. With two choice boxes, however, the period of delay could be increased from 2 to 4 seconds, to from 16 to 18 seconds. Cowan, using her own method, found delays, with the single animal used in the experiment, of about 30 seconds. Here two choices were possible. Adams, using the direct method, offering four possibilities of choice got delays of 16 hours with one animal.

Two researches Hunter (6) and Walton (19) report the use of dogs as subjects both of which used what we have designated above as indirect methods. Hunter reports that his dogs were able to delay and yet make a high percentage of correct responses one after 5 minutes and the other after only a 2 second period of delay. Walton on the other hand, reports delays of 1 minute with the four compartments and longer delays with fewer compartments.

Only one study appears in the literature which utilized raccoons as the experimental animal. This was the early study of Hunter (6). Using the indirect method employed in the study, he found that the raccoons could react correctly after delay when 3 boxes were used for from 3 to 15 seconds. When only 2 choices were possible the period of delay would be increased up to 20 and 25 seconds and still have a high percentage of correct responses.

Buytendijk (2) using a direct method in which he threw pieces of food to the monkey and then after the animal had grasped the first piece, threw several more pieces in other directions while the animal watched. The animal then ate the piece which he had grasped first and then in turn the other pieces. The whole procedure involved about 7 minutes before the last piece was eaten. This would indicate that the animal was able to remember the location of the food for that period of time at least. Nellmann and Trendelenburg (15) using the method involving the hinged box found that if a delay of from one-fourth to one-half minute was allowed to intervene between the turning of the box and the

release of the animal, it was no longer able to remember which side to approach to open the box. Tinklepaugh (17) on the other hand, using the direct method found delays of from a few seconds to from 15 to 20 hours.

As in the case of monkeys so in the case of chimpanzees, several different methods have been applied in the study of the delayed response. All of these methods however, would appear to fall into our classification of the direct method. Kohts (13), with her chimpanzee, Iona, used the method of "choice from sample" and found that if more than 15 seconds was allowed to elapse between the removal of the sample and the selection of the correct object to match the sample the animal was unable to make the correct choice. Yerkes and Yerkes (25) using the method described under "Methods" above found that their chimpanzees could delay up to 3 or 4 hours with 4 choice boxes present. When, however, the experiment was varied so that the absolute and relative position of the boxes was eliminated the animals made no delays longer than 30 minutes but the authors state that they had not yet reached the limit of the animals ability. In the same study the authors report experiments in which the food was buried in a large pasture. The animals apparently were able to react correctly to the buried food situation after a period of 48 hours. Kohler (10) in his early experiments on the chimpanzee reported delays in buried food experiments after intervals of $16\frac{1}{2}$ hours.

Yerkes (23) in studying the gorilla made some experiments on delayed reaction also using the technique and situations described above under "Method" found that the animal could delay in the situation involving the four choice boxes after a delay of 3 hours. In buried food experiments the animal reacted correctly after 48 hours.

Apparently only two studies have been done on young children. Both of these studies were done by Hunter (6) (7), one using a modification of his apparatus involving the indirect method and the other using a direct method. With the indirect method, he obtained correct responses from the children after delays of 25 minutes. In the study involving the direct method (7) Hunter

tested a child younger than any of the children tested in the previous experiment. Here he found that the child could delay for from 20 to 24 seconds and still make a fairly high percentage of correct choices.

From the foregoing summary of the length of delays that the various animals were able to bridge and still make the correct response, it is apparent, if we assume that all experimenters exercised the proper controls in their experiments, that the period of delay which the animal can make is dependent upon a considerable number of factors. It appears that the length of delay is a resultant of among other factors, (1) the type of apparatus used; (2) the method employed; (3) the species of animal used; (4) the kind of association utilized; and so on. We shall consider these later.

2. Factors producing correct post-delay responses. As was indicated above, the most important problem from the standpoint of psychology, which arises in the delayed response experiment is not how long can the animal delay after the removal of the stimulus and still make the correct response, but rather, upon what basis is it possible for the animal to react to a stimulus which was present for a time and then is removed. From analysis of the problem Hunter (6) concludes that there are at least two possibilities: (1) the animal may maintain gross bodily orientation or orientation of some part of its body during the delay and then when released react on the basis of that orientation. Or perchance, the animal may by chance recover the proper orientation just at the moment of release and thus still react on the basis of that orientation; or (2) the animal might react, providing it does not maintain orientation, on the basis of some intra-organic factor which is non-observable by the experimenter. If the animal can be shown not to react on the basis of orientation, then this intra-organic cue must be present. If it is, it might again, according to Hunter, be either sensory or imaginal. In either case it would perform the function of serving as a representation of the stimulus, i.e., it would be ideational in its functioning.

Accepting Hunter's analysis of the problem the necessity then

arises to determine whether or not an animal solves the delayed response situation by maintaining gross bodily orientation or the orientation of some part of the body, and if it does not, to determine the intra-organic cue which the animal used as a basis for its reaction. Furthermore, it must be determined, if possible, what the nature of this cue is, that is, whether it is sensory or imaginal. Another problem which would naturally arise from the above would be whether or not the sensory or imaginal element remains in the highest level of nervous activity, namely, in attention during the delay or if it does not under what conditions it can be reinstated. We shall now consider what evidence has been gathered from the behavior of the animals in previous experiments which throws light on these problems.

The experimental evidence produced by Hunter (9) and Ulrich (18) would indicate rather clearly that at least in the type of situation which they used, if the rat can delay at all, it does so on the basis of the maintenance of gross bodily orientation. If the bodily orientation of the animal was disrupted during the period of delay the animal could not make more correct responses in a series of trials than could be accounted for on the basis of chance. Furthermore, the evidence from Yarborough's (21) experiment on cats would indicate likewise that when the cat is placed in a situation and handled by a method similar to that used by Hunter the animal when it reacts correctly does so by the maintainance of overt bodily attitudes. Hunter's (6) experiment again would indicate that the dog in order to react successfully after a period of delay, must do so by means of the maintainance of the orientation of some part of the body.

On the other hand, evidence that animals make use of intra-organic cues in their reactions in the delayed response situation are not lacking. Hunter (6) found that in the case of his raccoons and children correct reaction could be made without the maintainance of overt bodily attitudes. In this case, not only was the orientation not maintained in the release box, but even after release, the animal might change its orientation and still make the correct response in more cases than could be accounted for on the basis of chance reactions. Furthermore, the behavior

of the animal was such as to indicate that it was not making a chance reaction. Other experimenters, such as Cowan (3) and Adams (1) using different situations and methods have presented evidence that with cats also the correct reaction can take place after delay without the maintainance of overt bodily attitudes. Providing their experiments were properly controlled their evidence would indicate such to be the case. Walton (19) working with dogs in an apparatus somewhat similar to that of Hunter's found that when he rotated the release box the animal was able to react correctly. He concludes that the dog does not react on the basis of orientation but as Hunter (7) has pointed out the evidence which he presents does not entirely exclude such a possibility. It merely shows that the orientation could be disturbed during the delay period. It may, however, have been reinstated before the animal was released from the box. Hunter seems to think that if such were the case it would be a separate problem that should be investigated. To the present author such does not seem to be the case. The mechanism which must be functioning in this case could quite easily be the same mechanism that is functioning when the animal reorients after it has once left the cage. All experimenters who have worked on this problem utilizing monkeys (Buytendijk (2), Nellmann and Trendelenburg (15), and Tinklepaugh (17)), chimpanzees (Kohler (10), Yerkes and Yerkes (25), and Kohts (13)), the gorilla, Yerkes (23), and Children (Hunter (6) (7)) cite results which indicate that these animals in various situations can react on the basis of some non-observable intra-organic cue.

If then, the foregoing species of animals can react on the basis of an intra-organic cue, the question remains concerning the nature of this cue. Is it sensory or is it imaginal? Watson (20) in discussing Hunter's results from his earlier experiment agrees with Hunter's main analysis of the possibilities of solution, but differs with Hunter on the basis for explanation of the raccoon's behavior. According to Hunter (6) they reacted on the basis of what he called sensory thought, which was largely a reinstatement of kinaesthetic processes. According to Watson there is "no known mechanism" whereby the behavior could be

explained except on the basis of the use of language habits pro-
vided a delayed response situation was actually present. Since
such habits were out of the question Watson seems inclined to
believe that the animals may have been reacting on the basis of
sensory stimulation. This was written before Hunter had
performed his experiment on his own daughter and shown that
even though language habits had not yet developed the child
could still react after delay. In a review Hunter (9) has pointed
out the possibility of a reinstatement of kinaesthetic processes
which might function as a representation of the stimulus, and on
this basis again concludes after the study of his daughter, Thayer,
before language habits had developed, that the intra-organic
cue may be present even though language habits have not yet
developed and furthermore, that these cues are in all probability
a reinstatement of kinaesthetic processes.

Another factor producing correct post-delay response and
which should be mentioned here is the position factor. Yerkes
and Yerkes (25) in their study of the chimpanzee, Yerkes (23)
on the gorilla, and also to some extent Tinklepaugh (17) in his
study of the monkey found the correct delay after reaction was
facilitated when the postion factor was not eliminated from the
situation, i.e., when the food container was placed in the same
position after the period of delay that it had been in during the
period when the animal saw the experimenter place the food in
the container the animal reacted correctly to the situation with
greater facility than when the position of the container was
changed during the interval of delay. In the case of the study
mentioned above made by Yerkes and Yerkes, they found that
when this position factor was removed and when the animal had
to react on the basis of some other differential factor in the sit-
uation the animal became at first, entirely incapable of correct
response. After a period of training, however, the animal ac-
quired some ability to react on the basis of a differential quality
other than position, in this case the color of the box. These
experimenters conclude that such response involves, in all prob-
ability representational processes, "probably they are symbolic;
possibly, imaginal" (p. 269). Tinklepaugh, again in the study

mentioned above found that in the case of one of the animals investigated it was apparently reacting on a basis of position in relation to itself whereas the other animal was reacting rather on the basis of the position of the container in relation to the rest of the environment.

In Tinklepaugh's study an attempt was made to study further the nature of the representative factors used by his animals. In order to throw further light on the subject he employed the "substitution method" which had been used in studying memory formerly by Buytendijk in his study of the monkey. By this method the animal is permitted to see the experimenter place food under or in the food container. During the absence of the animal or during the period of delay, the experimenter replaces the kind of food which the animal saw placed under the food container by some other kind of food or he places the food in some other container. The animal then returns and the behavior is observed. When Buytendijk used this method he observed that the animal on returning to get the food expressed *"etonnement"* when it was not found in the original postion and then went to the other container and secured it. In Tinklepaugh's experiment the substitution of food was used with this method. The foods substituted were bananas and lettuce. If the experimenter permitted the animal to see banana placed under the food container and then during the period of delay, substituted lettuce for the banana, the animal after delay approached the container lifted it and failing to find the banana expressed, as Tinklepaugh states it, "disappointment." If later lettuce was placed in the container while the animal was watching the process and the animal was removed from the situation for a period of delay and then returned it went to the container and ate the lettuce. But if now, at a later time, the substitution was tried again, but this time the animal saw the experimenter place lettuce in the container and during the period of delay banana was substituted for it, the animal returned and without hesitation, at least as far as Tinklepaugh points out, or "etonnement" picked up the banana and ate it. In the present writer's estimation experiments of this kind are of little value in studying the nature of whatever

intra-organic cues are present, for the difficulty of properly controlling the situation is extreme. Before such evidence can carry much weight the experimenter must determine definitely the preferences of the animal and at the same time must produce evidence that the animal has not become habituated to obtaining a certain type of food in the experiment. To a certain extent similar behavior can be shown in the rat. If it is accustomed to receiving one kind of food in the problem situation and is then given another it is likely to hesitate and in some cases would refuse entirely the new kind of food.

From the foregoing summary of the experimental work which has been done by previous workers on the delayed reaction problem, it may be seen that many variations of both the direct and the indirect method have been used not only with different species of animals but also with the same species. It is obvious that we can make no direct comparisons of the results obtained in these various situations. In general, the situations have been varied not with respect to single conditions but with respect to several conditions each of which seems to have a direct relation to the length of delay obtained. An examination of these studies indicates that the determining conditions include the method, direct or indirect, the mode of behavior for the animal permitted or favored by the experimental conditions, the species of animal studied, and especially when the indirect method is used, the number of choices offered to the animal, the criterion of proficiency which determines how long the animal will be trained on one period of delay before being advanced to a longer period as well as the criterion for satisfactory evidence of having attained the maximal length of delay that is reported.

III. DESCRIPTION OF ANIMALS USED

During the entire course of this study thirty albino rats (20 males and 10 females) were used. These were bred from animals selected at random from the colony used in the Department of Psychology at the University of Minnesota. The animals used in the experiment were taken from several litters and were marked and numbered when they were separated from the mothers. The

original numbers assigned to the rats are given in Table I under "original number." Later, to facilitate identification, the animals were assigned the numbers given in the column under

TABLE I

Table showing cues used, sex and age at starting on experiment

ORIGINAL NUMBER	EXPERIMENTAL NUMBER	SEX	AGE AT WHICH STARTED
			days
8	V-1	Male	72
9	K-1	Male	73
10	K-2	Male	74
11	V-2	Male	76
12	V-3	Female	91
13	K-3	Female	93
14	K-4	Female	104
15	V-4	Female	99
16	K-5	Female	132
17	A-1	Male	86
18	A-2	Male	86
19	A-3	Female	102
20	A-4	Female	102
21	V-5	Female	145
22	A-5	Male	81
23	V-6	Male	82
24	K-6	Female	149
25	K-7	Male	95
27	K-8	Female	105
29	V-7	Male	94
30	V-8	Male	94
31	V-9	Male	153
32	A-6	Male	153
33	A-7	Male	153
34	V-10	Male	153
	1	Male	100
	7	Male	98
	9	Male	125
	13	Male	96
	15	Male	200

"experimental number." We shall use these latter numbers in referring to them throughout this manuscript. The letters preceding the numerals refer to the type of cue used with the animal throughout the experiment, namely: A indicates auditory

cues; K indicates kinaesthetic cues; and V indicates visual cues. In those cases where no letter precedes the number of the animal, i.e., with nos. 1, 7, 9, 13, and 15, visual cues were used. All of the experimental animals were normal, healthy, vigorous rats and were maintained as such throughout the experiment except in a few cases where the animal had to be dropped due to having developed internal ear disease or some other sickness. The actual age in days when each animal was started on the problem is also included.

<div align="center">IV. APPARATUS AND GENERAL METHOD</div>

<div align="center">*A. Apparatus*</div>

The apparatus used in this experiment consisted of a box 22 inches in width and 140 inches in length and covered with wire netting of $\frac{1}{4}$ inch mesh. It was constructed of $\frac{1}{2}$ inch boards for all side and interior walls while the floor was made of regular 1 inch flooring. All side and interior walls were 6 inches high from the surface of the floor. The entire box was mounted upon legs 22 inches by 2 inches by 1 inch. This made a substantial piece of apparatus. The box consisted of six sections as shown in figures 1 and 2. Section A, consisting of the food box and the release box, R, was 12 inches long and 22 inches wide. The release box, which was 5 inches wide and 6 inches long, was placed against the middle portion of the wall separating Section A from Section B. The remainder of Section A served as the food box. From the release box, R, a sliding door (a) $3\frac{1}{2}$ inches in height and 4 inches in width served as an entrance from the release box to Section B of the apparatus. Entrance from the side alleys into the food box was effected by means of two doors (b and c) 3 inches wide and $2\frac{3}{4}$ inches high. These doors were made of tin and swung into the food box thus permitting the animal to enter the food box from the side alleys but not vice versa. These doors were swung from the top and each was equipped with a small bolt which held the door firmly closed when the animal was not to be permitted to enter the food box from that side.

The remainder of the apparatus consisted of sections B, C, D, E, and F. Each of these sections was divided by means of two walls, into three alleys, a middle alley 12 inches wide and two side alleys each 4 inches wide. Sections B and C were each 22 inches in length while Sections D, E and F were each 28 inches in length. The walls separating the alleys mentioned above were in each case 4 inches shorter than the length of the section of which they were a part. This arrangement gave two exits one into each
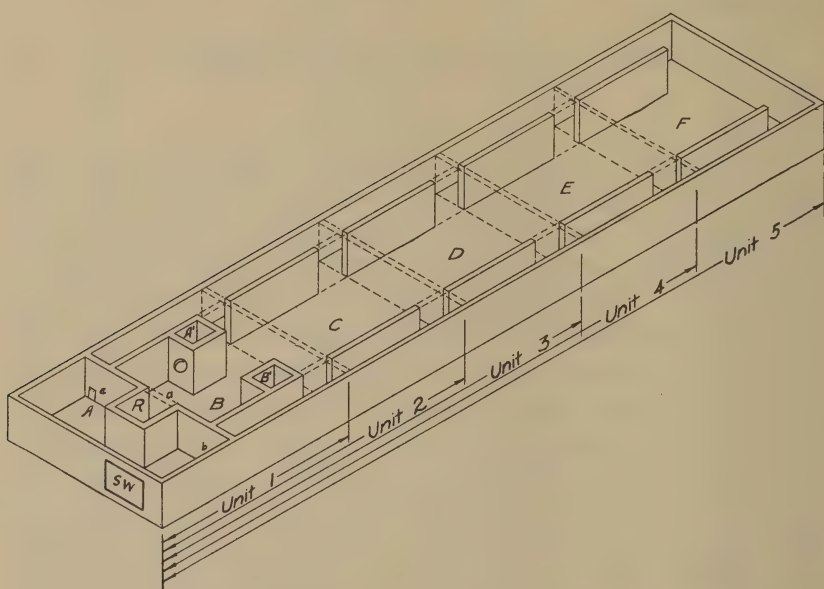
Fig. 1. Perspective View of Apparatus

side alley at the end of each section. These exits could, however, be closed easily by means of sliding doors which fitted evenly into the wall thus making the alleys continuous from Section B to the end of the apparatus in Section F.

In the middle alley of Section B were two boxes A' and B'. These were stationary and were each 4 inches wide and 5 inches long and of the same height as the walls separating the alleys. As will be seen from the diagram, these boxes narrowed the middle alley from 12 inches to 4 inches at the point where the animal

had to "choose" the particular alley leading to the food com-
partment.   Behind the boxes, were two narrow alleys 4 inches
wide formed by the rear wall of the boxes and the wall separating
Sections B and C.

The wall separating Sections B and C was movable and could
be placed at will at the end of any section desired in the position
indicated by the dotted lines in the diagram.   When placed at
the end of any section, all exits from the middle alley except
those adjacent to the end-wall itself were closed by means of the
sliding doors mentioned in the second paragraph of this section.
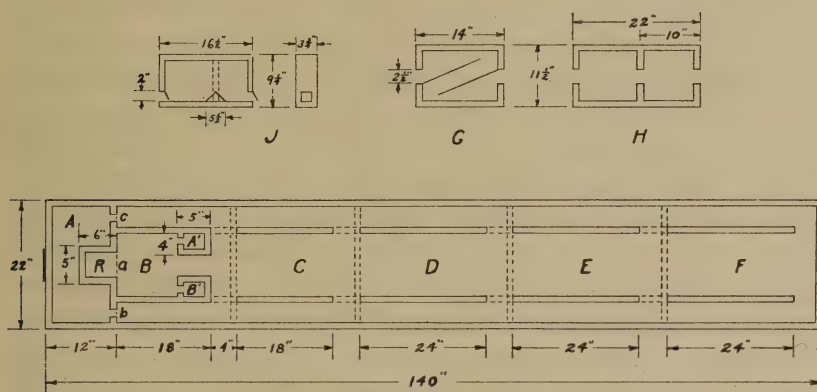
Fig. 2. Floor Plan of Apparatus

This movable wall made it possible to increase the alleys (both
middle and side) to the following lengths.   When the movable
wall was at the end of Section B the middle alley (from the release
box to the point where the animal had to "choose" the side alley
leading to the food box) was 18 inches in length.   When it was
placed at the end of Section C the middle alley was 40 inches in
length; of Section D 68 inches in length; of Section E, 96 inches
in length; and of Section F, 124 inches in length.   These dis-
tances were always measured to the middle point of the middle
alley and 4 inches from the end-wall since the exits were 4 inches
wide and the animal could leave the middle alley at any point
within that distance.

Two additional parts of the apparatus (G and H) are shown at the side of Sections D and E respectively in the accompanying diagram. (G) was used for the purpose of (1) making more difficult or perhaps preventing the following, by the animal, of one or the other of the side walls in passing back through the middle alley; (2) breaking up any tendency which the animal might have to keep its body bent to one side when going down the middle alley; and (3) still further lengthening the path between the giving of the cue and the final "choice" of the side alley. It consisted of a box 14 inches long and $11\frac{1}{2}$ inches wide with an entrance $2\frac{1}{2}$ inches wide and 5 inches high at the middle of one end and an exit of the same size at the opposite end of the box. When it was in position in the apparatus, it fitted nicely into the middle alley, the floor of which also served as the floor of the box. Within the box were two walls each 11 inches long, made of tin and placed diagonally as shown in the diagram. These walls extended from the side of the entrance to the point $2\frac{1}{2}$ inches from the opposite side and the opposite end-wall thus causing the animal to bend its body sharply once toward each side in passing through the apparatus. Since the box was floorless, whether the animal was directed to the right or left on entering could be determined by merely inverting the box. In order to make this possible both the upper and lower ends of the entrance and the exit were semi-circular and placed so as to be raised $\frac{1}{2}$ inch from the floor.

Apparatus (H) shown in the diagram at the side of Section E was a box constructed of the same $\frac{1}{2}$ inch material as the rest of the apparatus. It was 22 inches long and $11\frac{1}{2}$ inches wide with a floor made of strong cardboard reinforced with tin strips. It was divided into two compartments by a cross-wall. The first of these compartments was 12 inches long while the second was 10 inches long. The entrance as well as the exit to this box consisted of a circular opening $2\frac{1}{2}$ inches in diameter which at its lowest point was $\frac{1}{2}$ inch above the floor. The opening in the wall between the two compartments was $2\frac{1}{2}$ inches wide and $2\frac{1}{2}$ inches high, the sides being straight and the top semi-circular. This box could be placed in the middle alley of Section D, E, or

F.   Furthermore, it could be used with or without any additional impediment.   As will be mentioned later on, with several animals, sand was piled up before the wall separating the compartments, hiding the opening and forcing the animal to dig through the sand before being able to reach the place where the path leading to food must be selected.   The box without the sand compelled the animal to take a certain path at a certain part of the run, with sand in the box it accomplished the further purpose of forcing the animal to go through an additional series of movements and also lengthened the time of delay.

There now remain to be described certain pieces of apparatus that were used in presenting the cues to the animal, cues, on the basis of which the animal must make its selection of the path leading to the food.   Of these, as stated previously, there were three, namely; visual, auditory, and a combination probably involving both the kinaesthetic and tactual senses.   We shall describe these in order.   The visual cues were supplied by means of two 15 Watt-120 Volt Mazda bulbs contained one in each light box (A' and B') in Section B.   In the front side of each of these boxes was a circular opening $1\frac{1}{4}$ inches in diameter backed with opal glass.   The current was obtained from the regular city current and was directed into the desired light by means of a double throw knife switch situated at the front of the apparatus. When auditory cues were used, they were supplied by means of a small electric buzzer fastened to the wall of the room in which the experimentation was done.   This placed the buzzer to the left of the apparatus about 4 inches from it, on a level with it and about midway between the release box and the farther end of Section B of the apparatus.   The current for the buzzer was supplied from a dry-cell battery.   It was controlled by means of a knife switch fastened to the front of the apparatus itself.   This was the only direct connection between the buzzer and the apparatus.   The third type of cue could be supplied by means of the piece of apparatus (J) shown in the diagram.   This consisted of a practically light tight box $16\frac{1}{2}$ inches long, $3\frac{3}{4}$ inches wide and $9\frac{1}{4}$ inches high.   It was constructed of $\frac{1}{2}$ inch material and had all joints lined with felt.   The entrance and exit consisted of

square openings 2 inches by 2 inches and located $\frac{1}{2}$ inch above the floor of the apparatus. The entrance and exit were closed by means of two tin swinging doors slightly larger than the openings which were lined with black felt. The felt prevented light from entering around the doors as well as serving to prevent noise caused by the doors' striking the wood after the rat had passed. The door at the entrance swung inward and upward being fastened to the inside of the box whereas the one at the exit was fastened to the outside and swung outward and upward. The lid of this box was removable and had attached to it two arms made of angle iron and extending from the lid to the floor. These arms fitted into two grooves one on each side of the inside of the box and just deep enough to present an even surface when the lid was in place. These grooves were located $5\frac{1}{4}$ inches from the farther end of the box. On the floor of this box could be placed a right angled incline which had a $5\frac{1}{2}$ inch base and which was $2\frac{3}{4}$ inches from the base to the apex. This increased the absolute length of path by $2\frac{1}{4}$ inches and in addition required the animal to ascend and descend an incline which was at the most a 45 degree angle. The incline was held stationary while in the box by means of a steel pin through the apex which fitted into two notches in the arms attached to the top of the box. When the incline was in position in the box, it fitted tightly on the floor and extended over the whole width of the box. The entire box when in position in the apparatus fitted into Section B and extended from the entrance of B back between and about an inch beyond the front of the light boxes.

In each side alley were kept two sets of grills each set covering the floor space of the alley for one section. These grills were moved with the movable end-wall so that if the apparatus was set for an alley longer than two sections the last two sections always had grills in them. The current for these grills was furnished from the city light current which was passed through a resistance coil and then through a small 110 Volt Bell Transformer (with primary and secondary reversed) before it reached the grills. The current could be directed into the grills on one side or the other as desired by means of a double-throw knife switch, again attached to the front of the apparatus.

All wooden and tin parts of the apparatus were painted black both inside and outside. The apparatus was placed in a room which could be darkened though it was not completely dark. The left side of the apparatus was adjacent to and only about four inches from the wall of the room. Directly above Section B was an indirect lighting fixture containing an electric light bulb (15 Watt) and covered by a paper shield to subdue the light still further. This was the only source of artificial light in the room while experimentation was going on except the light which was derived from the stimulus lights in the apparatus.

B. General method

The animals used in the experiment were kept in the regular animal room in the Psychological Laboratory. Each day before the experimentation took place all of the animals that were being used were taken to the experimental room at the same time and remained there until the conclusion of the day's experimentation. The experimental room was a rectangular room 13 feet, 6 inches in length and 7 feet, 6 inches in width and was located on the second floor of the laboratory. This necessitated carrying the animals quite a distance each time before the research for the day was conducted but they soon became accustomed to it and, in all probability, were not affected adversely by it. During the course of the experiment the animals were fed on the regular mash diet which has been in use at the University of Minnesota Psychological Laboratory.

The general method used throughout this research was as follows: Associations were first set up between some form of stimulus or cue, (i.e., a light, a sound, or certainki naestheticcues), certain movements when the end of the alley was reached and food. When the animal had learned to discriminate between the two forms of the cue used so that it went to the food box in the right direction when the appropriate stimulus was given, that is, when it had met the arbitrary criterion of such learning which we had set, the distance between the point where the cue could be received last and the end of the alley where the animal had to choose the alley leading to food was lengthened thus increasing

the length of the period of delay in making the final reaction to the stimulus originally given. When this length of alley had been mastered, again to the point where the animal met our criterion of learning, the alley was lengthened further, again increasing the period of delay. Our original plan was to continue this method of lengthening the path between the place where the cue was given and the place where the selection of paths had to be made, until we found a distance over which the animal could no longer learn to make the correct response to a given cue. It was soon discovered, however, that this would require a piece of apparatus longer than our space permitted. Consequently it was decided to train the animal in this manner until it reached the end of Section F of the apparatus and from that point increase the period of delay by the introduction into the middle alley of various impediments such as those described under "Apparatus G and H."

More specifically the actual training of the animal involved the following procedure. Several days before the actual training was to be begun the animal was taken to the experimental room and was placed in the apparatus and was permitted to explore the entire box for a short period. It was then fed in the food box. This was repeated on following days until the animal appeared to be reacting normally. All the swinging doors at the ends of the side alleys (and, when the box for giving the kinaesthetic cues was to be used on the animal, these swinging doors also) were kept open until the animal, when released from the release box, went through the apparatus to the food box without hesitation. The swinging doors were then put down and the animal was trained to go through these before it received food. When it had learned to do this without hesitation the regular training series were begun.

During the actual training series, the movable end-wall which fixed the length of alley was placed in the proper position then the animal to be trained was put in the food box of the apparatus. If the animal was to be given the cue to turn to the left for example, the small bolt holding the right door leading from the right side alley to the food box was pushed into position so as to

lock the door. The switch directing the current into the grills was set so that the current would flow into the grills on the right side of the apparatus. The animal was then placed in the release box, R, and after a few seconds the sliding door (a) was raised, releasing the animal. At the same time that the door was raised, if the stimulus was to be either an auditory or a visual stimulus, that stimulus was turned on by moving the knife switch at the front of the apparatus controlling the stimulus. If a kinaesthetic cue was to be given the box for that purpose described under "Apparatus" was put into position before the rat was placed in the food box. The apparatus was then set for the cue for the left turn at the end of the alley and the animal was released as described above. If at the end of the alley the animal turned to the left and came back the left alley he found the swinging door unbolted and was given food in the food box. If, however, he chose the right alley, he received a shock and, if he continued to the end of the alley found the door locked and had to retrace to the end of the alley, cross the middle alley and go out the alley on the left side only to receive no food. In either case, the doors and the grills were again adjusted for the next trial and the animal was put through the same process until it had received its required number of trials for that day. This was the procedure that was followed regularly except that after the discrimination had been set up and the alleys had been lengthened, if the cue was either auditory or visual, the stimulus was turned off at the moment that the animal reached the point in the alley between the light boxes A' and B'. Furthermore, instead of turning the current into the grills before the animal was released, after the discrimination had once been set up and the alleys lengthened, the current was not directed into the grills at all unless the animal actually chose the wrong alley in which case the current was turned on the moment the animal stepped on the grill in the wrong alley. A careful record was kept of the correctness or incorrectness of the response, and, where the alley was lengthened, of the time consumed to the half second by the animal in going from the place where the stimulus was turned off to the point where he entered the side alley. This was done by means

of an ordinary stop watch which was started when the animal's body was between boxes A′ and B′. The watch was stopped when the animal indicated that it had made its choice by entering either side alley.

The experimental animals may be divided conveniently into three groups on the basis of the type of stimulus that was employed as a cue in the problem. With one group of animals a light stimulus determined the alley which was correct for the given trial and which, consequently, led to food. The stimulus in this case was given through the medium of the light boxes A′ B′ in Section B of the apparatus. If the light in A′ (on the left) was turned on when the animal was released, the opening in the front of the box backed by the opal glass appeared as a bright white circular area. This was the cue that the animal had to turn to the left in order to obtain food. If the lighted circular area appeared in the light box B′ the animal had to turn to the right at the end of the alley in order to obtain food when it reached the food box. With a second group of animals an auditory stimulus determined the alley which had to be selected if food was to be received in the food box. As previously pointed out under "Apparatus," this cue was supplied by means of a buzzer fastened to the wall of the room at the left side of the apparatus. If the buzzer was sounded as the animal was passing through Section B of the apparatus, it was the signal that the animal had to turn to the right at the end of the alley. If the buzzer was not sounded the animal had to turn to the left. The third group of animals were given a cue which differed from the first two groups in that the cue was intended to furnish a kinaesthetic cue rather than an auditory or visual stimulus. In order to present this cue to the animal, the piece of apparatus described as (J) under "Apparatus" was designed. As pointed out in that section the apparatus consisted of a box $16\frac{1}{2}$ inches long, $3\frac{3}{4}$ inches wide, and $9\frac{1}{4}$ inches high. It was practically light-tight with one door serving as an entrance and another as an exit and the animal passing through the box had to enter it immediately on leaving the release box. It had to pass through a swinging door at the entrance which closed immediately as the animal's body passed

through the entrance into the interior of the box. Here it had a straight narrow alley $15\frac{1}{2}$ inches long with a swinging door at the other end. If this alley was unobstructed the animal had to turn to the left at the end of the path after it left the box. If, however, in passing through the dark narrow alley of the box, the animal encountered the 45 degree incline it had, on leaving the box, to turn to the right when it reached the end of the alley. Since the box in which this stimulus was presented to the animal was practically light-tight it is not likely that the animal was able to get any visual stimulation when in the box either when the incline was in position or when it was not there. In addition to being practically light-tight itself, the box when in position in the apparatus was so placed in Section B that the entrance of the box fitted tightly against the wall separating Sections A and B. Thus the only light which could reach the entrance of the box had to come through the release box. Considering that this was painted black and that the room was already darkened, it is extremely unlikely that any light entered it at all since when the animal was entering its body filled the opening and when it was in, the door closed immediately behind it. In all probability, the only cues that it could get were those of kinaesthesis and perhaps of tact. It might be possible that as the animal was passing through the alley it hit the incline with the tip of its nose and, after it had passed through several times, it was stimulated as the vibrissae came in contact with the incline and thus in both cases received tactual stimulation. It is quite certain, on the other hand, that the animal received kinaesthetic stimulation. If the incline was in position in the box, the animal had to climb over the incline before it could pass out at the exit of the box. This again would give the animal some tactual stimulation due to the unequal pressure on different feet, but in all probability, the greatest amount of stimulation came from the stimulation of the kinaesthetic sense as the animal climbed or ran over the incline. All of these of course were absent when the incline was not in the box. In learning the discrimination, the animal had to learn then that when they were present it had to turn to the right on leaving the box or when it reached the end of

the alley and to the left when they were absent. There is another possibility of stimulation which would differ in the two conditions. When the incline was absent the actual length of path which the animal had to traverse within the box was $15\frac{1}{2}$ inches. When the incline was in position the shortest possible length of alley was no longer $15\frac{1}{2}$ inches but the presence of the incline increased this distance by the length of the 45 degree incline and the 45 degree decline minus the length of the base. The length of the incline plus the length of the decline was $7\frac{3}{4}$ inches while the length of the base was $5\frac{1}{2}$ inches. The presence of the incline in the apparatus thus increased the length of path by $2\frac{1}{4}$ inches. This too should produce some difference in the amount of kinaesthetic stimulation which the animal would receive under the two conditions. Since it is certain that kinaesthetic stimulation differed under the two conditions we shall hereafter refer to these cues as kinaesthetic.

From the foregoing it will be quite evident that the three groups of animals had to set up discriminations under conditions which were not exactly comparable. In the case of the animals receiving visual cues the animals had to discriminate not between the presence and absence of a particular stimulation but rather between the position from which that stimulation was derived. In the case of the visual cues this difference of position amounted to a difference of 8 inches from the center of the one stimulus to the center of the other. Or looking at the same thing from another viewpoint if the light came from the right of the animal, it had to turn to that side; if from the left, it had to turn to the left. It always had to turn toward the side from which the stimulation had come. In other words, a light was present each time that the animal passed through the apparatus. In the case of the group receiving the auditory cues this was not the case. Here the animal had to learn that the presence of the sound of the buzzer meant that the animal was to make one series of movements while the absence of that sound meant the making of another series of movements or turning to the opposite side. When the sound stimulus was used the animal had to turn not toward the side from which the stimulation came but away

from it. In the case of the animals receiving the kinaesthetic cues the animal again had to discriminate between the presence or absence of additional stimulation. Here the animal had to turn to the one side when the additional stimulations were present and to the opposite side when they were absent.

The animals of each group were given ten trials daily throughout the entire period of training. These trials followed one after the other with a short interval between trials. The experimentation throughout was done in the late afternoon between 3 and 6 o'clock for the most part. Furthermore, after an animal was once started, it was run at about the same time each day. This was possible by beginning experimentation promptly at a regular time and then running the animals each day in the same order.

Throughout the training the series of trials (whether the cue to turn to the left or to the right was to be given) was changed frequently in order to prevent the animal from learning the series.

One more point remains to be considered under the heading of "General Method." This is the question of how many or what percentage of correct choices shall be taken as sufficient to justify our saying that the animal has learned the discrimination, or as sufficient to justify moving the animal on to a longer period of delay. With only two possible choices of alley leading to the food box, pure chance should cause the animal to make 50 per cent correct choices over a long series of trials. But rarely in experimentation are a sufficient number of choices made for chance to operate as it should theoretically. If, however, we make the series sufficiently long and in addition to that require the animal to make a sufficiently high percentage of correct choices over a limited length of series we can be sure that the animal has learned the problem and is not merely reacting by chance. In this experiment we arbitrarily set the length of the series of trials at 40 (ten on each of four successive days) and set our standard of having learned at 85 per cent correct choices over the four days with no less than 80 per cent correct choices on any one of the four days. The animal might now react 85 per cent of the time correctly over the four day period but if any one of

the four days averaged less than 80 per cent correct the animal
was not said to have learned the discrimination or the particular
period of delay on which it happened to be and consequently was
continued until it finally did reach our criterion.

In December 1928 the training of the animals, 1, 7, 9 and 13
was begun using a somewhat different method from that used
with the aforementioned animals. The method with these ani-
mals consisted of setting up a discrimination with light as a stim-
ulus in exactly the same manner as in the case of the animals
designated by V (and the number of the animal), except that
with these four animals fifteen trials were given per day. After
the discrimination had been set up the animal was advanced to
the part of the apparatus which necessitated a delay between the
giving of the stimulus and the point where the choice of alley had
to be made, but here, instead of advancing the animal unit by
unit as the separate units were mastered, an attempt was made
to set up the four units involving delay periods simultaneously.
In order to do this after the discrimination had been set up, the
animal was given 15 trials per day as follows: three trials on the
discrimination followed by three trials on each of the four units
to the end of the apparatus. Here, as with the preceding ani-
mals, one trial followed after the other with a brief period inter-
vening. A somewhat longer interval intervened between the
last trial on one unit and the first trial on the next unit than
between the trials within the same unit in order to permit the
experimenter to make the necessary alterations of the apparatus
for the trials on the next longer unit.

With this group of animals we considered the discrimination
set up when the animal had on three successive days (45 trials)
made an average of at least 85 per cent correct choices with no
single day having less than 80 per cent correct. In the units
involving a period of delay the animal was to be continued in
training until the responses on all units reached a sufficiently
high percentage of responses to show that all were learned. In
all other respects these animals were treated as were the others.

In March 1929 animal 15 was started on the problem under
exactly the same conditions and with the same method except

that the stimulus for turning to the right consisted of one light in each light box while the stimulus for turning to the left was the absence of light in both boxes. In all other respects this animal's training was similar to that of the preceding four animals.

C. Controls exercised

One of the criticisms (Watson (20, p. 225)) that has been urged against the experiments on delayed reaction performed by Hunter has been that he did not control sufficiently the possibility that the box in which the light last appeared might have risen in temperature and again that even if the rise in temperature had not been sufficient for the animal to select on that basis the correct box yet the rise might have been sufficient to increase "the diffusion of gaseous particles which arouse the olfactory receptors" and give the animal a basis for its selection of the box. Of course in such a case no delayed reaction would be present but it would merely be a case of discrimination on the basis of the temperature or the olfactory sense or perhaps of a combination of the two. Due to the method and apparatus used in this research such possibilities were entirely avoided. Furthermore, if they were present at all in the first part of the training in the present problem they became more remote the further the animal progressed in the problem. Here the animal had to make the final reaction in the middle alley at a point where the light had never been.

Nor was it possible in this study that the animal could get any olfactory cue from the food which was used to reward the animal after a correct choice. In all cases the animal was fed in the food box and there only at the middle of the box directly in line with the middle of the release box and against the wall of the food box opposite that against which the release box was placed. Under the existing conditions any odor which the food might possibly have given off would have diffused equally into both alleys.

Neither could the animal get other olfactory cues for the only other possible ones would be the scent from its own body re-

maining stronger in the alley last used than in the other alley. If the animal were reacting on such a basis either a position habit of always going to the one side or a simple alternation habit would result. Either of these possibilities would mean that the animal could never meet the required criterion because the series used were always chosen so as to preclude such possibilities.

No possibility of the animal receiving a cue as to the correct alley to enter was possible as far as the grills in the side alleys were concerned. As has been previously stated the current in the grills was controlled by means of a switch at the front of the apparatus. As a result it was possible to direct the current into the grill desired immediately when the animal entered the wrong alley. As a result the animal had to make its selection of alley and enter it before any current was in the grill to serve as a cue.

Another possibility that needs to be controlled in animal experiments is that the animal may receive cues as to the correct response from the experimenter himself. Again the possibility of the animal's receiving such cues in this problem was rather remote. As far as visual cues are concerned, it has already been stated that the room was darkened. Furthermore, at no time when a period of delay was involved was the animal closer to the experimenter than 64 inches or over 5 feet and as he advanced further in the problem this distance increased until, when the animal reached the further end of the apparatus. it was at least 12 feet from the experimenter. It is not probable that the animal could get any visual cues from the experimenter at any of these distances. The animals were, however, tested at various times for this factor by placing a heavy cardboard screen between the experimenter and the apparatus. Under these conditions the animals reacted about the same as when the screen was absent, consequently the above suppositions appear correct. Furthermore, the behavior of the animals when they reached the end of the middle alley was such that it was highly improbable that they could be reacting to any auditory cues such as they might receive from the experimenter's breathing rate and so on.

One more possibility remains to be discussed under the general heading of Controls. That is the possibility that whenever a

delay was involved the animal made the discrimination in Section B of the apparatus all right but then followed either the right or the left wall of the middle alley to the end of the alley and then turned into the side alley on that side of the apparatus. That this was a real possibility was definitely shown by a number of the animals. In the early trials, on some sections of the apparatus, they definitely showed this form of behavior. After the first few trials on the section, however, this disappeared and the animal followed the same path in the alley irrespective of the cue that was given in Section B. Yet even this behavior did not preclude the possibility that the animal was following the wall on either side of the apparatus, for example, it might have continued looking at the wall toward which it had to turn when it reached the end of the alley. Though we could not entirely eliminate this possibility yet we could increase the difficulty of so doing. In attempt to effect such an increase in difficulty the apparatus (G) described under "Apparatus" was designed. This practically reversed the path of the animal for a short distance and, while it still did not eliminate the above possibility yet it certainly did add a confusing element to the situation. As is pointed out later the behavior of the animal while in the problem indicated that it was not following the side walls while going through the middle alley.

V. EXPERIMENTAL RESULTS

A. Setting-up the discrimination

As was mentioned before the experimental animals were divided into three groups:

(A) Those animals which were trained to respond to a light stimulus;

(B) Those animals which were trained to respond to a sound stimulus; and

(C) Those animals which were trained to respond to the presence or the absence of the incline.

Group (A) may further be divided into two sets of animals: (1) those receiving ten trials per day, and (2) those receiving 15 trials per day. Ten animals were used in Set 1 of Group A.

Set 2 of Group A consisted of 5 animals. These will be considered presently. Group B consisted of 7 animals and Group C of 8 animals. The various animals composing each group are designated in the following tables by V (and the number of the animal) for those animals trained to respond to the light stimulus; A (and the number of the animal) for those trained to respond to the sound stimulus; and K (and the number of the animal) for those trained to respond to the incline plane.

Table II gives the number of trials required for setting up the discrimination to the point where the individual animal met our criterion of learning which as stated previously was that the animal make at least 85 per cent correct responses over four consecutive days with no less than 80 per cent correct choices on any one of the four days. In addition to the number of trials required to attain this criterion, it also gives the percentage of correct choices made over the four day period. The records given in Table II are for only those rats which were given ten trials per day.

From the foregoing table it may be seen that all the animals learned the discrimination upon which they were being trained with the exception of rats K-2 and K-5. In the case of K-2 the animal averaged 45 per cent correct responses over the last 4 days training and 50.2 per cent over the entire 830 trials on the discrimination. At no time during the entire learning did this animal average more than 60 per cent correct over a four day period. During the greater part of the training the animal exhibited a position habit of always running into the right alley which probably accounted for its lack of improvement in learning the discrimination. At the end of the designated number of trials the animal was dropped from the problem. In the case of K-5, the average over the last 4 days was $52\frac{1}{2}$ per cent and the average over the entire 240 trials was $55\frac{1}{2}$ per cent. This animal was in very poor physical condition and since she was showing no progress, in all probability due to that factor, she was disposed of.

An examination of Table II will reveal a decided difference in the number of trials required by each group to learn the discrimination. Thus Group A which was trained to discriminate on the basis of light required on the average (median) 405 trials

to meet our criterion; Group B trained to discriminate on the basis of auditory stimulation required a median of 210 trials; while Group C, trained to discriminate on the basis of the presence or absence of the inclined plane required on the average

TABLE II

Table showing the number of trials required for learning the discrimination and the percentage correct over the last four days

	RAT	NUMBER OF TRIALS FOR LEARNING	AVERAGE PER CENT CORRECT OVER LAST FOUR DAYS
	V-1	670	85
	V-2	970	85
	V-3	720	$87\frac{1}{2}$
	V-4	620	85
Group A	V-5	180	$87\frac{1}{2}$
	V-6	710	90
	V-7	190	$87\frac{1}{2}$
	V-8	150	90
	V-9	140	90
	V-10	170	90
	A-1	410	$87\frac{1}{2}$
	A-2	180	$87\frac{1}{2}$
	A-3	210	85
Group B	A-4	350	$87\frac{1}{2}$
	A-5	240	85
	A-6	200	$87\frac{1}{2}$
	A-7	180	85
	K-1	1,300	$87\frac{1}{2}$
	K-2	830	45
	K-3	1,200	85
Group C	K-4	1,460	85
	K-5	240	$52\frac{1}{2}$
	K-6	890	100
	K-7	770	85
	K-8	600	90

1045 trials. This difference is, however, not to be explained entirely on the difference in difficulty between the three types of cues. These differences can be accounted for in part by the fact that punishment was not introduced at the same point in the learning for all rats. The first eight rats which were trained on

the discrimination were started without punishment, some of them having 600 trials in this manner. It then became evident that, if the discrimination was to be set up within a reasonable number of trials, punishment must be used. Accordingly it was introduced. With all other animals used in the experiment punishment was introduced on the sixth day, i.e., after the fiftieth trial. Table III shows for the animals with which punishment was not introduced on the sixth day, the number of trials each animal was run without punishment and the percentage correct over those trials; the number of trials with punishment, necessary to set up the discrimination and the percentage correct over

TABLE III

Table showing number of trials with and without punishment and records over those trials for those animals given more than 50 trials without punishment

RAT	NUMBER OF TRIALS WITHOUT PUNISH-MENT	PER CENT CORRECT WITHOUT PUNISH-MENT	NUMBER OF TRIALS WITH PUNISH-MENT	PER CENT CORRECT WITH PUNISH-MENT	TOTAL NUMBER OF TRIALS	PER CENT CORRECT FOR ALL TRIALS	PER CENT CORRECT OVER LAST 40 TRIALS
V-1	350	59.7	320	63.4	670	61.5	85
V-2	600	62	370	69.4	970	64.8	85
V-3	400	53.2	320	68.7	720	60.1	87.5
V-4	400	49	220	73.1	620	57.6	85
K-1	600	62	700	64.5	1,300	63.4	87.5
K-2	600	50	230	50.8	830	50.2	45
K-3	400	48	800	61.8	1,200	57.1	85
K-4	300	49.3	1,160	60.6	1,460	58.2	85

those trials; and the total number of trials necessary to set up the discrimination and the percentage correct over the total learning as well as the percentage correct over the last 40 trials.

If we eliminate the above animals and consider only those with which punishment was introduced on the sixth day of training, in other words after the fiftieth trial, we find that the median number of trials required for setting up the discrimination becomes 175 for Group A and 770 for Group C. The difference between the median number of trials required by Groups A and B, whether we include all the animals in group A or exclude those that were trained for a longer period without

punishment, is not sufficiently reliable to make possible any definite conclusion as to the relative ease of establishing the discrimination to a visual and to an auditory stimulus. The cases are too few, the differences which do appear are inconsistent in direction and the group which were trained without punishment were all trained on the visual cue and all required a relatively long time after the introduction of punishment to meet our criterion for the discrimination. However, from the fact that for three animals the visual discrimination was established in fewer trials than were required by the best of the animals to learn the auditory discrimination, there is some evidence that the visual discrimination may be set up somewhat more easily. The three best animals learning the visual discrimination required 140, 150, and 170 trials respectively with 90 per cent correct over the last forty trials in each case; while the two best animals learning the auditory discrimination required 180 trials each with $87\frac{1}{2}$ and 85 per cent correct respectively.

In the case of the animals in which the kinaesthetic cue was used there is little doubt but that the discrimination was established much less readily than in the case of the animals in which the visual and the auditory cues were used. The median number of trials for establishing the discrimination to the kinaesthetic cues, when we include only those animals in which punishment was introduced after 50 trials, is 770 and the best animal in that group required 600 trials to learn it, the percentage correct over the last 40 trials being 90.

Another factor which may account in part for the difference in the rate of learning between the visual and auditory groups is that in the case of the group given the visual cues the stimulus was to turn toward the side on which the light appeared. In the case of the other group, the auditory group, the animal turned to one side when the stimulus was present and to the other side when the stimulus was absent. Thus the visual group could react in part on the basis not only of light but of light plus position or location of light. With the animals using the auditory stimulus no such reactions on the basis of position was possible. While the writer is unaware of other experimental evi-

dence on this question yet one would expect that it would be a factor influencing the learning. As far as the animals trained on the kinaesthetic cues, or the inclined plane, are concerned, reaction on the basis of position was again out of the question because both cues, either presence or absence of the incline, were received at the same point in space, that is in the same position.

As pointed out above, five animals (Set 2, Group A) which were trained to discriminate on the basis of visual stimuli were given 15 trials instead of 10 trials per day. For four of these animals numbers 1, 7, 9, and 13 the light stimulus was presented under exactly the same conditions as for Set 1 under Group A. Punishment in the training of this set of animals was introduced on the fourth day, i.e., after the forty-fifth trial. This made the

TABLE IV

Table showing the number of trials and the percentage correct over the last 45 trials for rats receiving 15 trials per day

RAT NUMBER	NUMBER OF TRIALS ON LEARNING	AVERAGE PER CENT CORRECT OVER LAST 45 TRIALS
1	270	87
7	525	89
9	255	89
13	195	91

animals in this set and in Set 1 comparable as to the method of presentation of the stimulus and practically so as to the time of the introduction of punishment. The results for these animals are given in Table IV.

From the above table it may be seen that these animals required a slightly greater number of trials to learn the discrimination than did the animals trained under similar conditions but having only ten trials per day. The median here is $262\frac{1}{2}$ trials as compared with the 175 trials for the animals having ten trials per day. This is in agreement with the results found in other investigations, viz., that the smaller number of trials per day is more economical with respect to the number of trials required for the learning. In this case, however, the number of days required to set up the discrimination was about equal in both cases.

In an attempt to make the visual cue more nearly comparable with the auditory cue the discrimination between light and the absence of light was set up with one animal. In this case both of the lights in Section B of the apparatus were turned on as the cue for the animal to turn into the right alley, while the absence of light was the cue to turn into the left alley. As it was not intended to use this animal in the regular experiments but only to set up the discrimination in order to compare, for our own information, this method of setting up a visual discrimination with the single light method and with the method of setting up the auditory discrimination, an animal which was considerably older than the other animals was used. When, however, we found that the animal learned the discrimination with unexpected ease and rapidity, we decided to retain the animal for further training, i.e., for training on the delay. The record of this animal in learning the discrimination follows. The discrimination was set up in 7 days with a total of 105 trials which is considerably less than was required by any other of the experimental animals. The percentages of correct trials for each of the seven days of 15 trials each were as follows: 47, 53, 47, 67, 100, 100, 100.

B. Training involving a period of delay

After the discrimination had been set up, as pointed out previously, the training involving a period of delay was undertaken. Again, as noted previously, the discrimination was set up using only that part of the apparatus designated under "Apparatus" as Sections A and B or unit 1, i.e., the food and release box, and the section in which the stimulus was presented. For convenience, the training involving only Sections A and B will, hereafter, be referred to as Stage I of the training. When the apparatus was lengthened so as to make Sections A, B, and C a unit, we shall refer to the training on this unit as Stage II. Likewise, when Sections D, E, and F were added in order and cumulatively to the stage II unit, we shall refer to the training involved as Stages III, IV, and V respectively. As may be seen from the diagram in the Section under "Apparatus," Stage II involved training over a path increased by 22 inches from the point where

the selection of the path had to take place in Stage I of the training. Whereas, in Stages III, IV and V the path was increased by 50, 78, and 106 inches respectively over that of Stage I.

The training, involving the delay, may conveniently be divided into two parts (1) that involving the straight unimpeded central pathway of Stages II, III, IV, and V, and (2) that involving the additional apparatus designed to increase the complexity of the problem as indicated under "Apparatus." The results reported first will include only those on Stages completed with the central alley unimpeded or up to and including Stage V. Furthermore, for the present we shall deal only with those animals which received ten trials per day throughout the training.

1. Training involving a straight unimpeded central pathway: Group differences as shown by: (a) The best stage reached. Not all the animals using the different types of stimuli reached and completed (that is met our criterion on) the same number of Stages of the training. Table V shows for the three groups of animals trained on the visual, the auditory and the kinaesthetic stimuli, the stage completed by each animal, the percentage of correct responses over the last forty trials and the total number of trials given each animal with the central pathway unimpeded.

As may readily be seen from Table V, differences, as far as the stage attained was concerned, appeared between the various groups of animals. Thus four animals in Group A never completed more than Stage II; four animals completed Stage III; and two animals completed Stage IV. In the group trained with auditory stimuli, Group B, one of the animals completed only Stage II; one completed Stage III; one completed Stage IV and four animals of this group reached and completed Stage V. In the case of Group C it may be seen that two of the animals, for reasons mentioned previously, never completed Stage I; one animal completed Stage I but never met the criterion on a higher stage; three animals completed Stage II and two animals completed Stage III. When we compare the total number of trials given to each group and the stage successfully completed in that amount of training it is evident that the groups rank in

order of success in the problem as follows: (1) auditory, (2) visual, and (3) kinaesthetic.

b. *The number of trials necessary to attain the highest stage.* Another line of evidence which indicates differences between groups

TABLE V

Table showing the final stage of the apparatus completed by the animals of each group

	RAT NUMBER	STAGE	PER CENT CORRECT OVER THE LAST FOUR DAYS	TOTAL NUMBER OF TRIALS GIVEN THE ANIMAL ON THE UNIMPEDED PATHWAY
	V-1	II	$92\frac{1}{2}$	2,010
	V-2	II	$87\frac{1}{2}$	4,610
	V-3	II	85	1,400
	V-4	III	85	2,360
Group A	V-5	IV	85	3,360
	V-6	III	85	1,600
	V-7	III	$87\frac{1}{2}$	1,910
	V-8	II	$92\frac{1}{2}$	1,910
	V-9	IV	85	450
	V-10	III	85	560
	A-1	V	85	1,120
	A-2	V	85	1,030
	A-3	III	95	1,400
Group B	A-4	V	90	2,090
	A-5	V	$97\frac{1}{2}$	820
	A-6	II	$92\frac{1}{2}$	360
	A-7	IV	90	470
	K-1	II	85	4,440
	K-2			830
	K-3	II	90	3,790
	K-4	I	$92\frac{1}{2}$	4,020
Group C	K-5			240
	K-6	III	$87\frac{1}{2}$	3,030
	K-7	III	85	1,610
	K-8	II	85	1,660

receiving the different types of cue used, is that of the number of trials required by the various animals in each group to complete the stages on which the animal met our arbitrary criterion of learning. In Table VI are tabulated the number of trials re-

quired by each animal of each group to meet the criterion on each stage which the animal completed successfully. In the second column under the various stages are shown the percentages of

TABLE VI

Table showing the number of trials required to complete each stage of the apparatus and the total number of trials required to attain the highest stage reached

RAT	I Trials	I Per cent	II Trials	II Per cent	III Trials	III Per cent	IV Trials	IV Per cent	V Trials	V Per cent	TRIALS
V-1	670	85	630	$92\frac{1}{2}$							1,300
V-2	970	85	1,500	$87\frac{1}{2}$							2,470
V-3	720	$87\frac{1}{2}$	110	85							830
V-4	620	85	840	85	590	85					2,050
V-5	180	$87\frac{1}{2}$	740	$87\frac{1}{2}$	1,020	$87\frac{1}{2}$	1,030	85			2,970
V-6	710	90	350	$87\frac{1}{2}$	540	85					1,600
V-7	190	$87\frac{1}{2}$	50	85	740	$87\frac{1}{2}$					980
V-8	150	90	70	$92\frac{1}{2}$							220
V-9	140	90	110	$87\frac{1}{2}$	150	90	50	85			450
V-10	170	90	90	$87\frac{1}{2}$	120	85					380
A-1	410	$87\frac{1}{2}$	110	85	80	85	150	90	370	85	1,120
A-2	180	$87\frac{1}{2}$	40	85	670	90	90	85	50	85	1,030
A-3	210	85	670	90	390	95					1,270
A-4	350	$87\frac{1}{2}$	360	85	450	85	870	90	60	90	2,090
A-5	240	85	80	$87\frac{1}{2}$	300	$92\frac{1}{2}$	70	90	130	$97\frac{1}{2}$	820
A-6	200	$87\frac{1}{2}$	60	$92\frac{1}{2}$							260
A-7	180	85	90	85	150	85	50	90			470
K-1	1,300	$87\frac{1}{2}$	930	85							2,230
K-2											
K-3	1,200	85	1,290	90							2,490
K-4	1,460	85									1,460
K-5											
K-6	890	100	280	$87\frac{1}{2}$	1,860	$87\frac{1}{2}$					3,030
K-7	770	85	390	85	220	85					1,380
K-8	600	90	690	85							1,290

Group A .... { V-1 through V-10 }
Group B .... { A-1 through A-7 }
Group C .... { K-1 through K-8 }

correct choices which that animal made over the four day period on which it met the criterion. In the last column of the table are given the total number of trials required by the animal from the beginning of its training, i.e., from the beginning of learning

the discrimination, to the point where it met the criterion on the highest stage of the five which it completed.

It was sometimes necessary in the course of training to return an animal for a short period to a stage which it had previously successfully completed. Whenever such was the case those

TABLE VII

Table showing for each animal the cumulative number of trials for each stage completed

	RAT	I	II	III	IV	V
	V-1	670	1,300			
	V-2	970	2,470			
	V-3	720	830			
	V-4	620	1,460	2,050		
Group A	V-5	180	920	1,940	2,970	
	V-6	710	1,060	1,600		
	V-7	190	240	980		
	V-8	150	220			
	V-9	140	250	400	450	
	V-10	170	260	380		
	A-1	410	520	600	750	1,120
	A-2	180	220	890	980	1,030
	A-3	210	880	1,270		
Group B	A-4	350	710	1,160	2,030	2,090
	A-5	240	320	620	690	820
	A-6	200	260			
	A-7	180	270	420	470	
	K-1	1,300	2,230			
	K-2					
	K-3	1,200	2,490			
Group C	K-4	1,460				
	K-5					
	K-6	890	1,170	3,030		
	K-7	770	1,160	1,380		
	K-8	600	1,290			

trials on that earlier stage were not included in the number of trials shown in Table VI.

In Table VII are shown the same data on the number of trials required as are shown in Table VI, except that they are arranged cumulatively, i.e., include with the trials on any given stage the trials on all previously completed stages.

A consideration of the median number of trials required by each group of animals to meet the criterion on each stage, as obtained from Table VI shows us that for Stage II, the first in which a period of delay was introduced, those trained with the visual cue required 230 trials; those with the auditory cue 90 trials; and those with kinaesthetic cue 690 trials. If we use the median of the cumulative number of trials, i.e., include those trials on Stage I with those on Stage II, as obtained from Table VII, the medians for the three groups are: Visual 545; Auditory 295; and Kinaesthetic 1290. If we exclude those animals which were trained for a long period without punishment on Stage I, as was described in an earlier part of this Section, we find that the differences between the medians on Stage II decrease but remain in the same direction. The median number of trials for Group A is 110; for Group B 90; and for Group C 390. The differences, however, when we consider the cumulative trials, are changed in direction somewhat. The median for Group A is 245, for Group B is 295 and for Group C is 1170. The explanation for the inversion of Groups A and B lies in the fact that, with the exclusion of the four animals trained on the discrimination in part without punishment, the group trained on the visual cue made a slightly better record on Stage I than did the group trained on the Auditory cue. The difference was great enough to obscure the better record of the Auditory group on Stage II.

The number of animals included in the three groups on Stage III is somewhat smaller than the number on Stage II since four of the animals in Group A, one in Group B and three in Group C completed Stage II but not Stage III. The differences between the medians of the number of trials required by the three groups on Stage III are in the same direction regardless of the method of obtaining them. Using only the trials on Stage III we find that Group A required a median of 565 trials, Group B 345 trials and Group C 940 (obtained from two animals with 1860 and 220 trials respectively). Using cumulative trials we find a median of 1290 for group A; of 755 for Group B and above 1380 trials (the number of trials by the best rat) for Group C. Excluding the one rat in Group A trained without punishment which reached

Stage III, the median for Group A is 540 on Stage III alone and 980 for the cumulative trials. Both of these medians are higher than those for Group B and less than those for Group C.

On Stages IV and V too few animals are represented in any group except the Auditory Group to make any comparisons other than to say that it is obvious that the record of the Auditory Group in regard to trials, is superior to those of the other groups. If we compare the median number of trials given the animals of the three groups in which they either did or could have completed the five stages in which the central pathway was unimpeded (See final column Table V, p. 51), we find that the median number of trials for Group A is 1910, for Group B is 1030 and for Group C is 2345. Thus it may be seen that if animals from Groups A and C had completed the five stages, the median number of trials required to do so would have been greatly in excess of that required by Group B.

c. Delay period attained. A third indication of evidence corroborating group differences is found in a consideration of the median length of delay for each rat on the completed stages. The median length of delay considered here is the median delay on only the correct responses made during the forty trials during which the animal met the criterion for the given stage. These medians are shown in Table VIII.

The difference between the three groups in the length of the delay period appear to be greatest in Stage II. In that Stage the groups would seem to maintain the same rank order, using the longest delay period as best, that they showed in regard to the highest stage reached and the fewness of trials required successfully to complete each stage. Four of the seven animals in the Auditory Group had median delay periods of 2 seconds while the remaining three had delay periods of $1\frac{1}{2}$ seconds. In the Visual Group only three of the ten animals delayed 2 seconds and the remaining animals delayed $1\frac{1}{2}$ seconds. On the other hand, we find that in the Kinaesthetic Group two of the five animals delayed 2 seconds, one delayed $1\frac{1}{2}$ seconds and two delayed only 1 second.

In all other stages either the number of cases is too small or

no differences are apparent except that all but one of the four animals from Group B which completed Stage V attained a median delay period on that Stage of $3\frac{1}{2}$ seconds while only one

TABLE VIII

Table showing for each rat the median length of delay in seconds (correct responses only) during the four-day period in which the animal met the criterion of each stage

	RAT	II	III	IV	V
	V-1	2			
	V-2	$1\frac{1}{2}$			
	V-3	$1\frac{1}{2}$			
	V-4	2	2		
Group A	V-5	$1\frac{1}{2}$	2	$3\frac{1}{2}$	
	V-6	$1\frac{1}{2}$	3		
	V-7	2	2		
	V-8	$1\frac{1}{2}$			
	V-9	$1\frac{1}{2}$	$2\frac{1}{2}$	3	
	V-10	$1\frac{1}{2}$	2		
	A-1	2	2	2	3
	A-2	$1\frac{1}{2}$	2	3	$3\frac{1}{2}$
	A-3	2	3		
Group B	A-4	2	2	3	$3\frac{1}{2}$
	A-5	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$
	A-6	$1\frac{1}{2}$			
	A-7	$1\frac{1}{2}$	2	3	
	K-1	2			
	K-2				
	K-3	1			
Group C	K-4				
	K-5				
	K-6	$1\frac{1}{2}$	2		
	K-7	1	2		
	K-8	2			

animal (V-5) from either of the other groups attained a median delay of equal length on any completed stage.

From an examination of Tables VI, VII and VIII, it is evident that while there are consistent differences between the groups as shown by the medians of the number of trials, yet the differences within the groups are equally marked. These intra-

group differences are especially marked in Group A, somewhat less marked in Group C and much less evident in Group B. In Group A is found the animal (V-9) which up to the time when its training was unavoidably interrupted and discontinued had made the best record of any animal up to that point.[3] It is unfortunate that the training of this animal could not be continued for we have reason to believe on the basis of its record over the first four stages that it not only could have completed the five stages under consideration here, but would have advanced into the training involving at least some of the additional apparatus. In this group we find also on each stage involving delay an animal which shows either the poorest record or next to the poorest record of any animal in the three groups trained on that stage. While the number of trials required by the animals in Group C was in every case except one, above the median for the three groups yet the range of the number of trials for this group greatly exceeded the highest number of trials required by any one animal in Group B. In both Groups A and C, some individual animal required trials in excess of 1000 to meet the criterion on each of the completed stages involving delay; in contrast to this we find that no animal in Group B exceeded 870 trials to master any stage.

If we now consider those stages on which the animals received training without meeting our criterion, we find as shown in Table IX, that 16 of the 25 animals will be represented. For one animal the stage on which the uncompleted training was given was Stage II, for eight it was Stage III, for five it was Stage IV, and for two it was Stage V. Although for the last four days of training none of these animals showed a percentage of correct responses higher than 75 and that percentage in only two cases, yet for all except three of the animals the responses over the best four days showed 75 per cent or above and in three of the cases the per-

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[3] The training of Animals V-9, V-10 and A-7 was begun on June 2, 1928 in order to increase the number of animals trained on the first few stages. It was discontinued at the end of July due to the necessary absence of the experimenter from the laboratory. A-6 was started on the problem at the same time as the other three animals but died about the middle of July.

centage correct was 85 without meeting our criterion since on one of the four days the percentage had fallen below 80. These animals all show evidence of learning on the stages represented and, had our criterion for mastering the stage been slightly lower, most of these animals would have met it.

2. Training involving an impeded central pathway. The experiment was planned to include the introduction into the central

TABLE IX

Showing the results of training for those animals which received training without meeting our criterion on any of the first five stages

RAT NUMBER	STAGE	NUMBER OF TRIALS ON STAGE	LAST FOUR DAYS		BEST FOUR CONSECUTIVE DAYS	
			Per cent correct	Median delay	Per cent correct	Median delay
				seconds		*seconds*
V-1	III	710	55	4	$77\frac{1}{2}$	$2\frac{1}{2}$
V-2	III	2,140	$57\frac{1}{2}$	$3\frac{1}{2}$	$82\frac{1}{2}$	3
V-3	III	570	45	2	75	2
V-4	IV	310	60	5	$77\frac{1}{2}$	3
V-5	V	910	$62\frac{1}{2}$	5	$82\frac{1}{2}$	$6\frac{1}{2}$
V-7	IV	930	75	$3\frac{1}{4}$	$82\frac{1}{2}$	3
V-8	III	1,690	55	3	$77\frac{1}{2}$	3
V-10	IV	180	$47\frac{1}{2}$	3	$62\frac{1}{2}$	$3\frac{1}{2}$
A-3	IV	130	55	7	$67\frac{1}{2}$	6
A-6	III	100	75	$2\frac{1}{2}$	85	$2\frac{1}{2}$
K-1	III	2,210	$57\frac{1}{2}$	3	$82\frac{1}{2}$	3
K-3	III	1,300	$72\frac{1}{2}$	2	85	2
K-4	II	2,560	$62\frac{1}{2}$	$1\frac{1}{2}$	85	2
K-6	IV	960	$67\frac{1}{2}$	$3\frac{1}{2}$	75	$3\frac{1}{2}$
K-7	V	290	$57\frac{1}{2}$	3	70	3
K-8	III	370	$57\frac{1}{2}$	$2\frac{1}{2}$	80	3

pathway of various impediments (some of which are described as auxiliary sections under "Apparatus") as soon as the training of the animal on the first five stages had been completed. Not all of the animals included in the experiment received this additional training for the simple reason that the majority failed to advance sufficiently far to warrant its introduction. All of those animals which reached Stage V received at least some of this additional

training, and some of the other animals, which will be mentioned below, also received training on some parts of the additional apparatus before they had reached Stage V.

The first attempt to place an impediment in the central alley consisted of placing in it a very simple addition which would compel the animal to follow a somewhat different path there from that it had previously followed by its own selection. This consisted merely of two boards of 12 inches long and 6 inches high, placed crosswise and 4 inches apart in the middle of Section D of the apparatus. The animal had to pass through centrally located openings in the boards $2\frac{1}{2}$ inches wide by 5 inches high. The training involved with the introduction of this addition to the apparatus constitutes Stage VI. This stage differs from Stage V only by the inclusion of this impediment.

Three animals (A-1, A-2, A-5) were tested with this arrangement and completed the stage in a relatively short time, requiring only 50, 110 and 160 trials respectively with 85, 85 and $87\frac{1}{2}$ per cent correct responses over the last four days on the stage. Previously two of these animals, A-1 and A-2 had been running along the left side of the central pathway while the third, A-5, had been running along the right side, regardless of the cue given. The introduction of the two boards, of necessity interfered with those habits since the animal now had to go through the middle of the central pathway for at least 4 inches in Section D. This interference in the habitual reaction did not disrupt to any great degree the animal's ability to make the correct choice even though it increased the median period of delay over the last four days for A-2 from $3\frac{1}{2}$ seconds to $4\frac{1}{2}$ seconds and for A-5 from $3\frac{1}{2}$ seconds to 4 seconds. The median time of delay remained the same for A-1 which may afford a partial explanation for the smaller number of trials required by this animal to meet the criterion. When the boards were in position, A-1 showed a greater tendency to run through the middle of the central pathway throughout its entire length, whereas the other two animals still tended to run to the favored side up to the point where the boards compelled the animals to run in the middle of the pathway.

When the animals, A-1, A-2, and A-5, had again met our cri-

terion over four days under the conditions of Stage VI, a further change was made in the central alley. Two boards similar in size and location to those in Stage VI were employed, but now, instead of having the openings through the boards located centrally, they were located so that the animal entered the space between the boards at a point $2\frac{1}{2}$ inches from the one side wall of the central alley and left the space between the boards at a point $2\frac{1}{2}$ inches from the other side wall. That is, if the animal entered the space at the right side of the central alley, it left it at the left side of the alley and vice versa. This not only compelled the animal to use, at this part of the alley, a different path from that which it had used in Stage VI, but it also compelled the animal to turn at right angles twice in passing through the auxiliary apparatus consisting of the two boards. Furthermore, by reversing the two boards the experimenter could determine at will whether the animal was to enter the space between the boards on the right side of the central alley and leave on the left side or vice versa. In running the animals on this stage, which will hereafter be designated as Stage VII of the training, the animal was given three trials with the entrance at one side of the alley; the next four trials with the entrance on the opposite side from which it had been on the preceding three trials; and in the last three trials of the day the entrance was again in the same position as it had been in the first three trials. In each case the exit was on the side opposite the entrance. The following day's training was started with the boards in the same position as they were during the middle four trials of the preceding day's training. This method of changing the entrance from one side of the central alley to the other side after three or four trials compelled the animal to change his path slightly at this part of the central alley frequently.

In Stage VII we find that two of the animals, A-1 and A-2, required fewer trials to meet the criterion than did the third animal, A-5. These three animals completed this stage in 130, 100 and 430 trials respectively with $87\frac{1}{2}$, 90 and $87\frac{1}{2}$ per cent correct trials over the last four days. For A-1 the median delay period over the last forty trials on this stage increased by the

amount that is about typical as the animals go from stage to stage, viz., by 1 second or in this case from 3 seconds to 4 seconds. On the other hand, A-2, which completed this stage in fewer trials than did the other two, decreased the median delay period from $4\frac{1}{2}$ to 4 seconds, while A-5 which made the poorest record with respect to the number of trials required to meet the criterion made the best record on delay, the median delay for this stage being 6 seconds, an increase of 2 seconds over the median of Stage VI.

Since these three animals had given evidence, by their performance on Stages VI and VII, that their course within the central alley might be changed without making the situation so complex that the animal could no longer learn to react correctly, a still greater change of course was made by the introduction of the piece of apparatus described and diagramed as Section G under "Apparatus." This auxiliary apparatus when in position in the central alley was placed, as previously described under "Apparatus," in Section D with the entrance of Section G at the beginning of Section D and Section G extending into Section D for a distance of 14 inches. This additional apparatus again compelled the animal to enter Section D in the middle of the central alley and then directed the animal either to the right or left (as desired by the experimenter), to the corner of Section G where the animal had to change direction by bending the body sharply to one side and go for a short distance in the general direction from which it had come to the opposite corner of Section G where it was again compelled to bend the body sharply in the direction opposite to that at the last turn and then leave Section G in the middle of the central alley and slightly beyond the mid-point of Section D. The animal then went to the end of the apparatus where it made its selection of the path to the food box. The training with this setting will hereafter be designated as Stage VIII.

The same animals, A-1, A-2, and A-5, were trained with this arrangement of apparatus. As in the two preceding stages the three animals reached a point in the training where the correct responses for a given animal over forty trials averaged 85 per cent

or over, but one of the animals, A-2, did not reach the point within the training given where the correct responses on each day of the four day period were 80 per cent or above. On the third to the sixth days inclusive on Stage VIII the animal, A-2, averaged 85 per cent with a median delay period of 5 seconds, but it failed to meet our criterion for the learning since the percentages for the four days were 100, 90, 60, and 90. After that the animal did not again reach an average for any four days above 80 per cent. Thus for the period during which the animal was trained on this stage there seemed to be little or no improvement in learning. At the end of 560 trials on this stage, on the last 40 of which an average of only $57\frac{1}{2}$ per cent with a median delay of 5 seconds was made, the animal was returned to Stage V to test its performance when the central alley was unimpeded. At the end of 230 trials the animal had again met the criterion on Stage V with an average of $92\frac{1}{2}$ per cent correct choices and a median delay of 3 seconds.

The other two animals, A-1 and A-5, both met the criterion of learning on this stage, the former in 90 trials with 85 per cent correct choices and a median delay of $4\frac{1}{2}$ seconds, a delay $\frac{1}{2}$ second longer than that for the animal on Stage VII; and the latter in 210 trials with 85 per cent correct choices and a median delay of 7 seconds, an increase of 1 second over its delay on Stage VII.

Following the successful completion of Stage VIII, Rat A-1 was run on the apparatus with a different sort of arrangement. Section G was removed from Section D of the apparatus and Section H as described under "Apparatus" was inserted into Section E. When Section H was in place, it completely filled Section E so that the animal had to enter through the raised $2\frac{1}{2}$ inch circular entrance into the first compartment, pass through the $2\frac{1}{2}$ inch by $2\frac{1}{2}$ inch opening into the second compartment and out through a circular opening identical with the entrance. All three of these openings were in the middle of the central alley so that in this case again the animals were forced to follow along the mid-line of the central alley over a definite portion (22 inches) of the pathway. The training when this was in place in the ap-

paratus is designated as Stage IX when the animal has been trained successively on Stages VI, VII, and VIII following the successful completion of the first five stages and as Stage VA when the training on this follows immediately after the training on Stage V, i.e., when Stages VI, VII, and VIII have not intervened between the training on the apparatus so modified and that on the unimpeded central pathway of Stage V. Only A-1 was trained on Stage IX while A-1 again, A-2, A-4, and A-5 were all trained on Stage VA. The training on Stage VA will be reported after the results on the training on Stages IX and X have been reported.

A-1 completed Stage IX in 370 trials with 85 per cent correct responses and a median delay of 6 seconds over the last forty trials. Here again an increase of 1 second in the median length of delay over that of Stage VIII is found.

The next stage on which the animal A-1 was trained involved the same apparatus as Stage IX with the addition of sand piled before the opening of the cross partition within Section H which was in place in Section E. The animal had to dig through the sand to reach the end of the apparatus where it had to make the choice of the alley leading to food. Animal A-1 was successful in mastering this Stage. A record of 80 trials for learning with 85 per cent correct responses and a median delay of 7 seconds over the last 40 trials was made. Animal A-5 was also trained on Stage X but without having training on Stage IX; the training on Stage X in this case followed immediately after that on Stage VIII. This animal did not succeed in meeting our criterion in the 590 trials which it was given on this Stage. The record for the last 40 trials was 60 per cent correct responses with a median delay of 9 seconds while the record for the best consecutive four days was $82\frac{1}{2}$ per cent correct with a median delay of 11 seconds.

A greater complication than those described previously was then used in the training, that of including in the apparatus both Section G and Section H with the sand piled before the opening into the second compartment. Only one animal, A-1, which had successfully completed the entire sequence of stages

from I to X was trained on this next Stage, designated as Stage Xa. This animal was given 470 trials on this stage and over the last four days made $42\frac{1}{2}$ per cent correct choices with a median delay of 8 seconds. The best record for four consecutive days on this stage was made within 36 days of training with 80 per cent correct choices and a median delay of $11\frac{1}{2}$ seconds. Since the animal, although making 80 per cent correct choices over four consecutive days, i.e., 70, 80, 80, and 90 per cent respectively on the four days, failed to meet our criterion and at the end of the 470 trials appeared to be reacting on the basis of chance, it was returned to Stage IX for 130 trials. Failing to meet the criterion on that Stage within that number of trials, the last four days showing only 60 per cent correct responses with a median delay of 5 seconds and the best consecutive four days showing only $77\frac{1}{2}$ per cent correct responses with the same median delay, training on an impeded pathway was discontinued with this animal and the animal was returned to Stage I to be retrained to the point of again meeting the criterion on each of the first five stages and then to be advanced to Stage VA.

Four animals, A-1, A-2, A-4, and A-5, were trained on Stage VA. Of these, three (A-1, A-2, A-5) had been trained on Stages VI, VII and VIII and rat A-1 had in addition been previously trained on Stage IX, the apparatus for which was exactly like that for this stage. Although A-2 and A-5 had had no training on Stage IX, yet they had had training on a stage which bore considerable resemblance to this stage, namely Stage VI. In the training on Stage VI the animal, instead of being free to follow any path through the central alley, had to pass through centrally located openings in two boards placed crosswise and four inches apart in the middle of Section D, thus necessitating the use of a path through the middle of the central alley over, at least these four inches. In Stage VA, when Section H was in position in Section E, the animal again had to make use of a path through the middle of the central alley, but one which involved passing through three openings instead of two, one which was located nearer the end of the apparatus, the first opening was 56 inches from the end in Stage VA instead of 70

inches from the end as in Stage VI, and one which controlled
a longer portion of the path, 22 inches instead of 4 inches. In
Table X are found the records of all the animals trained on Stages
IX and VA.

It may be seen by reference to Table X that three of the
four animals which were run on Stages IX and VA were success-
ful in meeting the criterion on that stage. Two of these ani-
mals A-1 and A-2, required about the same number of trials for
learning Stage VA (450 and 470 trials respectively) but the
median delay periods varied from 5 seconds for animal A-1 to
7 seconds for animal A-2. Animal A-5 met the criterion on this
stage in 140 trials with 85 per cent correct responses and a median

TABLE X

*Showing the number of trials given, and for the last four days the per cent of correct
responses and the median delay on stages IX, and VA*

RAT NUMBER	STAGE NUMBER	NUMBER OF TRIALS	OVER LAST FOUR DAYS	
			Per cent correct	Median delays
				seconds
A-1	IX	370	85	6
A-1	VA	450	85	5
A-2	VA	470	85	7
A-4	VA	190	55	$3\frac{1}{2}$
A-5	VA	140	85	4

delay of 4 seconds. Animal A-4 was given only 190 trials on this
stage but on the last four days of that period made only $55\frac{1}{2}$ per
cent correct responses and on the best four days only $67\frac{1}{2}$ per
cent correct with median delays of $3\frac{1}{2}$ seconds and 4 seconds.
That animal was therefore returned to Stage I with the expecta-
tion of retraining it through the five stages before again making
the attempt to train it on Stage VA. This expectation was
frustrated, however, when, after the animal had relearned the
first four stages in fewer trials than it had required for the original
learning, it failed to meet the criterion again on Stage V even
after 1260 trials had been given on that stage.

The three animals which were successful in completing Stage
VA were then given training on the apparatus for that stage

with the addition of sand piled in Section H high enough to hide the opening through the middle partition. This training which will be referred to as Stage VAs, corresponds exactly to Stage X except for the training which preceded it. Animal A-1, which had learned Stage X in 80 trials with 85 per cent correct and a median delay of 7 seconds over the last four days with a record of 80 per cent correct choices with a median delay of $11\frac{1}{2}$ seconds on Stage Xa, was trained for 610 trials on this stage, but failed to meet our criterion of learning, having only $67\frac{1}{2}$ per cent correct responses with a median delay of $7\frac{1}{2}$ seconds over the last four days and only $77\frac{1}{2}$ per cent correct choices with a median delay of 7 seconds over the best four consecutive days. Animal A-2, which was given 530 trials on this stage, also failed to meet our criterion of learning, making a record of $72\frac{1}{2}$ per cent correct choices with a median delay of 8 seconds over the last four days and a record of 80 per cent correct choices with a median delay of $6\frac{1}{2}$ seconds over the best four days. The third animal, A-5, was trained for 410 trials on Stage VAs without making a record better than $77\frac{1}{2}$ per cent correct with a median delay of 9 seconds over 40 consecutive trials. It was then retrained on Stage VA which it relearned in 480 trials (90 per cent correct with a median delay of 4 seconds). When sand was then again piled before the opening in the partition of Section H, i.e., when training was resumed on Stage VAs, the animal met our criterion on the first four days with $87\frac{1}{2}$ per cent correct responses (80, 80, 100, and 90 per cent) and a median delay of $6\frac{1}{2}$ seconds. The sand was then piled higher and the training was continued with this animal until it should again meet our criterion, but this it failed to do during the 420 additional trials which were given. The best record which it made subsequent to the 40 trials on which it met the criterion was $82\frac{1}{2}$ per cent correct with a median delay of 7 seconds.

Of the animals which were trained with some impediment introduced into the central alley only those which were trained with Section H introduced before Stage V had been mastered, remained to be considered. When it became evident that these animals would not master the five stages within a period of time

that would permit training on Stages VA and VAs to be attempted, it was decided to introduce Section H into the apparatus as soon as the stage on which the animal was then being trained, had been mastered. Accordingly, for those animals Section H was placed in Section D, 28 inches farther forward than it had been placed for those animals trained on Stages IX and VA, and animals V-4, V-5, V-6 and K-7 were trained with it immediately following Stage III and animals V-9 and A-7 were trained with it immediately following Stage IV.

When the animals were trained on the apparatus including Section H immediately following Stage III the length of the path was not increased with the introduction of the section but remained the same as for Stage III and the training on this will be designated as Stage IIIA. Similarly the training on the apparatus, when Section H was included following the mastery of Stage IV, involved no increase in the length of path over that of Stage IV and it will be designated as Stage IVA.

Only two of the animals trained on Stage IIIA mastered it. Animal V-5 completed the stage in 580 trials with 85 per cent correct responses and a median delay of 2 seconds over the last four days. Animals V-4 and V-6 which failed to meet our criterion had 820 and 660 trials with $72\frac{1}{2}$ per cent and 60 per cent correct responses and a median delay of 2 seconds and 3 seconds respectively. The responses over the best four consecutive days show $77\frac{1}{2}$ per cent and $82\frac{1}{2}$ per cent correct choices with the same median delay periods as for the last four days.

Rats V-9 and A-7 were trained on Stage IVA following the completion of Stage IV but Rat K-7 was trained on Stage IVA following the mastery of Stage IIIA. In the case of animals V-9 and A-7 for which the training on Stage IVA included only 110 trials and 90 trials respectively, the training had to be terminated before they had completed the stage because of the absence of the experimenter. At the end of this training the record for the last four days for animal V-9 was 70 per cent correct choices with a median delay of 4 seconds, and for animal A-7 was $72\frac{1}{2}$ per cent correct choices with the same median delay. While on this stage Rat A-7 made no record better than that for

the last four days, Animal V-9 made a record of 75 per cent correct choices with a median delay of $4\frac{1}{2}$ seconds for the best four days. No conclusion can be drawn as to the probability that these animals would have met the criterion on this stage but we can point to the fact that these two animals had mastered the preceding stages in fewer trials than those required by any other animals.

Animal K-7 was the only one of those trained on Stage IVA that mastered it. For this 1890 trials were required and the record obtained was 90 per cent correct choices with a median delay of 3 seconds over the last four days. This animal was then advanced to Stage IVAs which corresponds to Stage IVA except that here again sand was piled so as to hide the opening through the partition in Section H. After 320 trials on this stage, in which the best record for four consecutive days was $77\frac{1}{2}$ per cent correct with a median delay of 5 seconds and the record for the last four days was 75 per cent correct with the same median delay, training was discontinued with this animal because of the absence of the experimenter from the laboratory.

From these results it will be noted that the best delay record was made on Stage Xa by animal A-1 with 80 per cent correct choices over a four day period with 70, 80, 80 and 90 per cent correct choices for the successive days. The median delay for this period was $11\frac{1}{2}$ seconds. This median was obtained from individual responses falling as follows: 2 at 10 seconds; 1 at $10\frac{1}{2}$ seconds; 12 at 11 seconds; 3 at $11\frac{1}{2}$ seconds; 9 at 12 seconds; 2 at 13 seconds; 1 at 26 seconds. The second best record also was made by an animal from Group B. This record was made on Stage X with $82\frac{1}{2}$ per cent correct responses obtained from the record of 90 per cent; 90; 70 per cent and 80 per cent for the successive days. The median delay of 11 seconds was obtained from the individual responses which were distributed as follows: 10 at 10 seconds; 2 at $10\frac{1}{2}$ seconds; 10 at 11 seconds; 7 at 12 seconds; 2 at 13 seconds; and 2 at 14 seconds. The best record for the animals in Group A is that made on Stage V by animal V-5 which showed an average of $82\frac{1}{2}$ per cent correct responses over four days, with 80, 90, 80 and 80 per cent correct on each of the four

days, and a median delay of $6\frac{1}{2}$ seconds. For Group C the best record was made by animal K-7 on Stage IVAs with $77\frac{1}{2}$ per cent correct choices over four days on which the individual days' records were 80, 90, 70 and 70 per cent correct with a median delay for the forty trials of 5 seconds.

The results for those animals trained with the various impediments placed in the central pathway are tabulated in Tables XI and XII. In Table XI is shown for each animal so trained all of the stages on which it received training together with the number of trials given on each stage, the percentage of correct responses and the median delay over the last four days, and the percentage correct and the median delay on any consecutive four days on the stage which show a record as good as or better than that on the last four days. In Table XII are shown the stages involving impediments in the central pathway together with the animals trained on each and their records as shown in Table XI.

These tables again bring out the superiority of the Auditory Group over the Visual Group and of the Visual Group over the Kinaesthetic Group both with respect to the number of animals reaching these stages and with respect to the number of stages mastered. For the convenience of the reader, Table XI presents for each rat all of the stages involving impediments on which the animal was trained. Table XII shows that four animals receiving the Visual cue, five receiving the Auditory cue and one receiving the Kinaesthetic cue were trained on the apparatus when Section H was included. The four animals receiving the Visual cue were trained with Section H in Section D of the apparatus and for three of these the path was only to the end of Section D or the same length as that for Stage III, while for the fourth it was to the end of Section E or of the same length as for Stage IV. Although in the aggregate these animals were given 2170 trials only one of them, one of those trained on the shorter path, mastered this stage. For one of the five animals with which the auditory cue was used, Section H was in Section D of the apparatus and the pathway was to the end of Section E while for the other four Section H was in Section E and the pathway extended to the end of the apparatus. With an aggre-

gate of 1340 trials for these five animals, three of them mastered the stage and all of these with the longer pathway, that extending

TABLE XI

Showing the animals, stages involving impediments on which each was trained, and results of such training

RAT NUMBER	STAGE	NUMBER OF TRIALS ON STAGE	LAST FOUR DAYS		BEST FOUR CONSECUTIVE DAYS	
			Per cent correct	Median delay	Per cent correct	Median delay
				seconds		*seconds*
V-4	IIIA	820	$72\frac{1}{2}$	2	$77\frac{1}{2}$	2
V-5	IIIA	580	85	2		
V-6	IIIA	660	60	3	$82\frac{1}{2}$	3
V-9	IVA	110	70	4	75	$4\frac{1}{2}$
A-1	VI	50	85	3		
	VII	130	$87\frac{1}{2}$	4		
	VIII	90	85	$4\frac{1}{2}$		
	IX	370	85	6		
	X	80	85	7		
	Xa	470	$42\frac{1}{2}$	8	80	$11\frac{1}{2}$
	VA	450	85	5		
	VAs	610	$67\frac{1}{2}$	$7\frac{1}{2}$	$77\frac{1}{2}$	7
A-2	VI	110	85	$4\frac{1}{2}$		
	VII	100	90	4		
	VIII	560	$57\frac{1}{2}$	5	85	5
	VA	470	85	7		
	VAs	530	$72\frac{1}{2}$	8	80	$6\frac{1}{2}$
A-4	VA	190	55	$3\frac{1}{2}$	$67\frac{1}{2}$	4
A-5	VI	160	$87\frac{1}{2}$	4		
	VII	430	$87\frac{1}{2}$	6		
	VIII	210	85	7		
	X	590	60	9	$82\frac{1}{2}$	11
	VA	140	85	4		
	VAs	870	75	$7\frac{1}{2}$	$87\frac{1}{2}$	$6\frac{1}{2}$*
A-7	IVA	90	$72\frac{1}{2}$	4		
K-7	IIIA	830	$82\frac{1}{2}$	2	85	2
	IVA	1,890	90	3		
	IVAs	320	75	5	$77\frac{1}{2}$	5

* After being trained on Stage VAs for 410 trials the animal averaged 65 per cent correct over the last 4 days. It was then returned to Stage VA for 480 trials when it met the criterion again for that stage; it was then returned to Stage VAs and averaged $87\frac{1}{2}$ per cent over the first four days on this stage. The sand was then piled higher with the result that the animal did not meet the criterion.

to the end of the apparatus. Neither of the two animals which failed to meet our criterion of learning on this stage had a num-

TABLE XII

Showing the stages involving impediments, the animals trained on each, and the results

STAGE	RAT NUMBER	NUMBER OF TRIALS	LAST FOUR DAYS		BEST FOUR CONSECUTIVE DAYS	
			Per cent correct	Median delay	Per cent correct	Median delay
				seconds		seconds
VI	A-1	50	85	3		
	A-2	110	85	4½		
	A-5	160	87½	4		
VII	A-1	130	87½	4		
	A-2	100	90	4		
	A-5	430	87½	6		
VIII	A-1	90	85	4½		
	A-2	560	57½	5	85	5
	A-5	210	85	7		
IX	A-1	370	85	6		
X	A-1	80	85	7		
	A-5	590	60	9	82½	11
Xa	A-1	470	42½	8	80	11½
VA	A-1	450	85	5		
	A-2	470	85	7		
	A-4	190	55	3½	67½	4
	A-5	140	85	4		
VAs	A-1	610	67½	7½	77½	7
	A-2	530	72½	8	80	6½
	A-5	870	75	7½	87½	6½*
IIIA	V-4	820	72½	2	77½	2
	V-5	580	85	2		
	V-6	660	60	3	82½	3
	K-7	830	82½	2	85	2
IVA	V-9	110	70	4	75	4½
	A-7	90	72½	4		
	K-7	1,890	90	3		
IVAs	K-7	320	75	5	77½	5

* See footnote on Table XI.

ber of trials sufficient to indicate whether or not it could have learned the stage to the point demanded by our criterion. It should be pointed out that the three animals, A-1, A-2, and A-5,

which mastered Stage VA had all been trained on more stages than had animals A-4 and A-7 which failed, in the length of training given, to meet our criterion. The mere fact of having met a larger number of successively varied conditions may be a factor which makes for greater adaptability to the problem at hand. These data which have been presented here do not answer that question but they do suggest that it should be considered. This difference in the number of varied conditions on which the animals had been trained also exists between animals A-1, A-2, and A-5 and those which were trained with the visual cue. As to the age of the animals when they were put on the Stage involving Section H there is little or no difference among the three groups.

Only one animal that was trained with the kinaesthetic cue was advanced to a Stage involving an impediment in the central pathway. This animal was given training on Stages IIIA and IVA both of which it mastered, but only after a very long period of training, the number of trials for Stage IIIA being greater than that given to any other animal trained on this Stage and the number for Stage IVA being more than twice that given on Stage IIIA.

The other comparisons which might be made from this table have been given in the preceding text and need not be repeated here.

3. *Training involving trials on five stages daily.* In the training of the animals, whose results we have been considering above, it was found that in the great majority of cases when the animal had completed one stage of the training and was advanced to the following stage which involved the addition of a new section of the apparatus, the change in situation was sufficient to bring about a disruption of the correctness of the animal's responses on the new stage of training. Thus a greater or lesser period of training was necessary before the animal again responded correctly to the cues that were given in the new setting of the apparatus. The new stage of training not only involved a new situation, at least at the end of the central alley for any particular stage of training, but it also involved an increase in the period of

delay between the place where the stimulus was given and the place where the final selection of the side alley leading to food had to be made. The question now arose as to whether this added amount of training was necessitated as a result of the new situation at the end of the alley, or whether it was due to the increase in the length of delay period necessary to reach the point of choice, or perhaps to a combination of these two factors. In other words, if the newness of the situation were eliminated would not the animal be able to respond with about as high a percentage of correct responses at the end of Stage V as he did at the end of Stage III with other conditions as nearly constant as we could make them? Or stated still differently would the animal meet our criterion first on Stage II and then the other stages successively or would all stages be mastered at the same time?

The particular method of training which we adopted to throw some light on this problem was as follows: The discrimination was set up in the usual manner except that 15 trials per day were used instead of ten as with the animals described above. Fifteen trials per day was selected so that after the discrimination had once been set up, we might give the animal three trials each day on the unit of the apparatus used in setting up the discrimination and three trials per day on each of the units involving delay, i.e., three trials per day on the second stage; three trials on the third stage; three trials on the fourth stage and three trials on the fifth stage, thus distributing the training of each day equally over the entire five stages.

The animals selected for this part of the experiment were the animals designated as numbers 1, 7, 9, and 13. It should be borne in mind that the discrimination was set up with these animals in the same manner as it was with the group of animals designated by V (followed by the number of the animal) except that these animals received 15 trials per day instead of 10 trials. The method of training as described above was used with these animals as soon as they had met the selected criterion on the discrimination, with the results as shown in Table XIII.

Inspection of Table XIII shows that the four animals trained

in this manner benefited little from the training they received. Animal 1 received training for 104 days with three trials on each stage on each day making a total of 312 trials on each stage; Animal 13 received training for 108 days making a total of 324 trials on each stage; Animal 7 received training for 72 days with a

TABLE XIII

Table showing records of animals trained on five stages daily

RAT NUMBER	STAGE	FIRST EIGHT DAYS		LAST EIGHT DAYS		BEST EIGHT DAYS		TOTAL TRAINING		
		Per cent	Median delay	Per cent	Median delay	Per cent	Median delay	Trials	Per cent	Median delay
			seconds		*seconds*		*seconds*			*seconds*
1	I	63		67		92		312	78	
	II	58	2	58	2	88	2	312	68	2
	III	50	3	46	3	71	3	312	59	3
	IV	50	4	38	4	79	4	312	61	4
	V	58	6	46	5	63	5	312	60	5
7	I	79		75		67		216	65	
	II	54	3	54	2	79	2	216	62	$2\frac{1}{2}$
	III	46	3	58	3	71	3	216	58	3
	IV	58	4	63	5	58	4	216	60	4
	V	54	5	54	$5\frac{1}{2}$	71	7	216	56	5
9	I	67		83		79		228	69	
	II	46	$2\frac{1}{2}$	71	2	71	2	228	57	2
	III	58	$4\frac{1}{2}$	63	$2\frac{1}{2}$	71	3	228	58	3
	IV	58	$6\frac{1}{4}$	46	$3\frac{1}{2}$	67	$4\frac{3}{4}$	228	58	$4\frac{1}{2}$
	V	54	7	54	5	75	6	228	54	$6\frac{1}{2}$
13	I	67		96		96		324	80	
	II	63	$2\frac{1}{2}$	75	2	75	2	324	66	$2\frac{1}{2}$
	III	54	3	67	3	71	3	324	65	$3\frac{1}{2}$
	IV	54	4	67	$4\frac{1}{2}$	75	4	324	57	5
	V	46	5	71	$5\frac{1}{2}$	88	5	324	63	6

total of 216 trials on each stage; while Animal 9 was trained for 76 days with a total of 228 trials on each stage. It will be seen from the table that the results are tabulated in units extending over a period of eight days. This length of period was selected because the series used was of such length that the same series was repeated on every eighth day. Furthermore, this length of

period should give us a series of trials (24 over eight days) on each stage which would be somewhat comparable to the 45 trials over three days which were used in our criterion for having learned the discrimination in the case of the same animals.

Inspection of Table XIII, especially the results for the first eight days of training, will show that as soon as the situation was changed so as to give the animal only three trials each day on the discrimination stage its reactions, which previously had shown that it had learned to discriminate between the stimuli given, fell off to roughly 67 per cent correct trials in 24. This was not unexpected for the change in situation would be sufficient to produce such disruption. The results on the other stages are in all cases so low that they represent, as was to be expected again since the animal here was in a new situation, chance reactions. If the records of the last eight days of training are examined from the second main column of the Table XIII, it will be seen that in the cases of Rats 1 and 7 the table shows no improvement. The reactions on the first or discrimination stage are of about the same percentage of correctness as they were over the first eight days nor is any improvement evinced on any of the other stages. The reactions are still what we should expect from chance. In the cases of animals 9 and 13 improvement on the first stage is in evidence though it is not what one might expect after the period of training which the animals had received. It is, furthermore, very doubtful if the records on the other stages show any real improvement over the records of the first eight days. If they do show any such improvement it is more in evidence in the case of Rat 13 than in the case of Animal 9. The best records for the animals are given in the third main column of the table. These indicate some degree of improvement in the case of Animal 13 but the improvement is slight and in all probability not very reliable due to the small number of trials involved. Just why these animals did not learn to react with a higher percentage of correct responses is not shown by the original data except that all of the animals showed a decided tendency to alternate from one side to the other in reacting in that part of the apparatus involving a period of delay. Furthermore, three

trials per day on each stage did not seem to be sufficient to break up this habit of alternation. Of course, there are numerous other factors which might play a part in preventing improvement but the experimenter believes the above to be the principal reason for non-improvement under the conditions used.

When it became evident, after about two months of training with the above animals, that little or no improvement was taking place, the experimenter suspected that an element of confusion might be responsible for the establishment of the alternation habit. That is, when the two light stimuli were used, the stimulus to turn to the right was so nearly the same as the stimulus to turn to the left (the only difference being in their location) that after a period of delay the animal confused the two. It might, then, be able to respond correctly when the cue was still in evidence but after a period of delay could not do so. What would be the result if the difference between the two stimuli were made more pronounced? In order to test this, for our own satisfaction and not expecting to use the animal in the regular experiment, we selected an animal which had been kept for use but had never been started in the problem. We set up the discrimination with this animal using the following cues: when both the lights in the two stimulus boxes were turned on, it served as the cue for the animal to turn to the right; when neither of the two lights was on, it was the cue that the animal was to turn to the left. As previously reported (Cf. Section on "Setting Up Discrimination") this animal under the above conditions learned the discrimination much more rapidly than we expected and its behavior was such that we decided to continue it for at least part of the training on delay. It was given exactly the same training as the preceding four animals except for the conditions noted concerning the presentation of the cues. The results from this one animal indicate that our supposition was not entirely unfounded as is shown in Table XIV.

Examination of this table and a comparison of the results given there with those given in Table XIII for the other four animals with which the same method was used, will reveal a decided difference between the behavior of the various animals reported

in the latter table and the animal reported in the table above. It will be remembered that this animal on the last three days of setting up the discrimination had a record of 100 per cent over that period. In the above table it will be seen that this animal when put under training in the new situation maintained 83 per cent correct responses on the first stage over the first eight day period whereas the other four animals with the exception of one animal, no. 7, averaged only from 63 to 67 per cent. The difference on the remaining stages which were entirely new for the animal remained somewhat more above chance, although only slightly so, than in the case of the other four animals. The chief difference between the two tables comes out of a considera-

TABLE XIV

Showing the percentage correct, and the median delay over the first, the last and the best eight days of training and over the total training of rat 15

STAGE	FIRST EIGHT DAYS		LAST EIGHT DAYS		BEST EIGHT DAYS		TOTAL LEARNING		
	Per cent	Median delay	Per cent	Median delay	Per cent	Median delay	Trials	Per cent	Median delay
		seconds		*seconds*		*seconds*			*seconds*
I	83		100		100		120	91	
II	67	$3\frac{1}{2}$	92	$1\frac{1}{4}$	92	$1\frac{3}{4}$	120	80	2
III	58	$4\frac{1}{4}$	92	$2\frac{1}{2}$	92	$2\frac{1}{2}$	120	70	3
IV	63	5	79	$3\frac{1}{2}$	79	$3\frac{1}{2}$	120	64	4
V	67	5	79	$4\frac{1}{2}$	79	$4\frac{1}{2}$	120	68	5

tion of the results over the last eight days and the best eight days of the learning in each case. Although the animals reported in Table XIII had almost three times as many trials as the animal reported in Table XIV, yet their record in no instance equalled or nearly equalled the record of this one animal. While the evidence from this one animal is not sufficient to show conclusively that the lack of improvement on the part of the four animals mentioned above was due to too great similarity of the cues used yet it greatly strengthens the supposition that such was the case.

The record for the first eight days presented in the table above shows furthermore, that during that period the animal under

consideration reacted with about the same degree of correctness on all of the four new stages of training. The record over the last eight days, on the other hand, indicates that when the same number of trials are given on the different stages the same amount of progress in the learning is not in evidence, but that the progress made on the shorter stages, i.e., those involving the shorter period of delay and the shorter path, takes place more rapidly than does that on the longer stages. This difference is shown quite plainly in the column showing the percentage of correct responses on the various stages throughout the whole training. There we find that in general, increasing the amount of delay decreases the percentage of correct responses regularly when the training has

TABLE XV

Showing the percentage correct and the median delay over the first, the last and the best eight days of training and over the total training

STAGE	FIRST EIGHT DAYS		LAST EIGHT DAYS		BEST EIGHT DAYS		TOTAL LEARNING		
	Per cent	Median delay	Per cent	Median delay	Per cent	Median delay	Trials	Per cent	Median delay
		seconds		*seconds*		*seconds*			*seconds*
I	100		100		100		144	99	
II	92	2	96	2	96	2	144	93	2
III	79	$3\frac{1}{2}$	92	3	96	3	144	85	3
V	67	$6\frac{1}{2}$	71	6	75	6	144	70	6
VS	63	10	83	$9\frac{1}{2}$	83	$9\frac{1}{2}$	144	65	9

involved the same number of trials on each length of period of delay.

After the animal had received 40 days training on each of the five stages given in the above table a change was again made in the arrangement of apparatus. Using the same number of trials per day, the animal was now trained over stages I, II, III, V, and VS. The change merely involved the elimination of Stage IV and the addition for the last three trials of the day, of Stage VS. This last stage consisted of Stage V with the addition of Section G (previously described) placed in Section D. The results from training with this setting of apparatus follow in Table XV.

It should be pointed out at this place that in Tables XIV and XV, when we speak of the "total learning" we refer only to the total learning with the five stages of the apparatus arranged as in the part which we have under consideration in the particular table involved. For example, when we say above that in the total learning the animal had 99 per cent correct responses, we mean that with this setting of the apparatus the animal had that percentage of correct responses and not over the total training that he has had on the discrimination.

Table XV shows that during the first eight days of the training with the new setting of the apparatus the animal continued to react 100 per cent correct on the first stage or, in other words, the change involved in the last part of the apparatus did not affect the reactions on the first part at all. The reactions on the second stage again give the same percentage of correct responses, namely 92 per cent, which were found for the animal's reactions on that stage during the last eight days shown in the previous Table. In the case of Stage III, however, it will be noted that whereas on the previous eight days with the other arrangement of apparatus the animal made 92 per cent correct choices, on the first eight days of the present arrangement the percentage of correct choices dropped to 79 per cent. In Stage V a similar drop is to be noted. In this case the percentage fell from 79 to 67 per cent. The drop in percentage of correct choices on these two stages is probably to be accounted for by two factors, the first of which is a result of the second. In the first place the period of delay was increased by 1 second in Stage III and by 2 seconds in the case of Stage V. This increase in the period of delay involved would seem to account for the drop in percentage of correct responses, for during the preceding eight days the animal had a median delay of $3\frac{1}{2}$ seconds on Stage IV and made 79 per cent correct choices with that length of delay. In the present arrangement the delay of that length comes in Stage III and we find the same percentage of correct responses. The increase in period of delay on Stage III over the period of delay on that stage with the preceding arrangement of apparatus is, in all probability, due to the fact that Section G, the new addition

to the apparatus, was placed in Section D when it was used as a part of Stage VS. It will be recalled that Section D was the part of the apparatus at the end of which the animal turned into the side alley when it was being trained on Stage III. Even when Section G was not in Section D, when the animal had to pass through Section D it ran slower and more cautiously as though it remembered that changed conditions existed in that part of the apparatus. This same factor accounts, in all probability, for the increased period of delay in Stage V over that found previously.

In Table XV again we note that on the last eight days of the training with this arrangement of apparatus there is a slight inversion in the percentages correct on Stages V and VS. This in all probability is to be accounted for by the fact that the last trials of the days always come on Stage VS and, due to the period of no training which followed it, it was learned more rapidly.

During the training of Rat 15 one more change of apparatus was made when the animal had reached the point of learning as shown for the last eight days in Table XV. This change consisted of dropping out the training on Stage II and training the animal on the following stages: I, III, V, VS, and VSA. Stage VSA consisted of the entire length of apparatus to the end of Stage V with the Section G in Section D of the apparatus as for Stage VS and the addition of Section H (previously described) in Section E of the apparatus. The procedure was again the same as before. The results are summarized in the following Table XVI. This animal was given only 16 days training with this arrangement of apparatus and then had to be dropped from further training due to a discontinuance of the experimental work on all of the animals concerned in this study.

Table XVI shows the results of all the training which this animal had with this particular arrangement of apparatus. The training period here was too short to show any results as far as improvement on this arrangement was concerned. It is of value, however, in that it (especially the results of the first eight days) may be compared with the last eight days of the

training with the preceding arrangement of apparatus. When this is done, we find that there has been a slight drop in the per cent of correct responses on the first stage. On Stage III which is now the second in the series in time we have 92 per cent correct responses with a median delay of $3\frac{1}{2}$ seconds. During the last eight days of training with the preceding arrangement of apparatus the animal on Stage III made 92 per cent correct responses with a median delay of 3 seconds. Thus the change of position in the series when delays involving this period of time were required did not seem to effect the correctness of the responses.

TABLE XVI

Showing the percentage correct and the median delay over the first and the last eight days of training and over the total training

STAGE	FIRST EIGHT DAYS		LAST EIGHT DAYS		TOTAL LEARNING		
	Per cent	Median delay	Per cent	Median delay	Trials	Per cent	Median delay
		seconds		*seconds*			*seconds*
I	96		96		48	96	
III	92	$3\frac{1}{2}$	96	3	48	94	$3\frac{1}{2}$
V	79	7	75	$6\frac{1}{4}$	48	77	$6\frac{1}{2}$
VS	54	11	54	8	48	54	9
VSA	67	10	58	9	48	60	10

If now we consider the results from this animal's training with respect to the period of delay involved but without regard to the Stage of training on which that length of delay was attained or the arrangement of the apparatus as the training proceeded, we find that, in general, with an increase in the length of delay there is a decrease in the percentage of correct responses. These results appear in Table XVII.

4. The influence of length of delay on the correctness of response. The report of the results obtained on the various stages on which the animals were trained in this study has indicated the records of these animals not only as to the percentage of correct responses but also as to the median length of delay on each stage. It is apparent from Tables VII, VIII, XI, XIII, XIV, XV, and XVI that the length of delay for the correct responses as shown by the

median is related to the length of the path over which the animal had to run and to the complications introduced into the path but that the proportion of the correct responses to the total number of responses for each interval of delay in seconds is not indicated. Further analysis of the data obtained in this study indicates that a relationship between the length of the delay period and the percentage of correct responses exists. Evidence for the existence of this relationship is shown in Tables XVIII, XIX and XX

TABLE XVII

Showing the total number of responses and the percentage correct for various periods of delay

LENGTH OF DELAY	TOTAL NUMBER OF RESPONSES	PER CENT CORRECT
No delay*..	105	73
No delay..	417	90
No delay..	297	95
1 second..	43	98
2 seconds.......................................	225	89
3 seconds.......................................	232	78
4 seconds.......................................	184	73
5 to 6 seconds..................................	226	63
7 to 8 seconds..................................	150	69
9 to 10 seconds.................................	85	61
Over 10 seconds.................................	80	61

* The total number and the percentage correct for the period of "no delay" are presented in three forms: first, the 105 responses which were originally required for setting up the discrimination; second, the total number of responses on Stage I including those for setting up the discrimination; and third, only those responses on Stage I when five different stages were being given each day.

in which only data obtained from the animals which were trained on ten trials per day are considered. Furthermore, in these tables are given all the responses of a given duration of delay made by the various animals of each Group regardless of the Stage on which they were made or of the period of the training in which they were made.

It will be noted from Table XVIII that for Group A, which was trained with the visual cue, the highest percentage (73.6) is for the 1 second delay period and that there is a progressive de-

crease in the percentage correct through 5 seconds. With delays above 5 seconds there is considerable fluctuation in the percentage correct which is probably due to the fact that the number of trials at each delay period is too few even for chance to operate if the animals, as we suspect that they did, were as a group reacting merely on the basis of chance. If we combine the trials from 5 seconds to 13 seconds inclusive so that we get a number of trials about equal to that at 4 seconds (2389 trials at 4 seconds and 2087 trials at 5 seconds to 13 seconds inclusive)

TABLE XVIII

Showing for the 10 animals in Group A, Set I, the total number of responses, the number correct and the percentage correct at each interval of delay in seconds

INTERVAL OF DELAY	TOTAL NUMBER	NUMBER CORRECT	PER CENT CORRECT
seconds			
1	1,665	1,226	73.6
2	6,838	4,560	66.6
3	6,581	4,136	62.8
4	2,389	1,356	56.7
5	1,034	557	53.8
6	448	260	58.0
7	205	116	56.5
8	147	75	51.0
9	87	49	56.3
10	77	47	61.0
11	31	20	65.5
12	35	20	57.0
13	23	14	61.0
Over 13	130	65	50.0

we find that the percentage correct is slightly below that for 4 seconds or is 55.4 and that for the trials on which there was a delay of more than 13 seconds the percentage decreases to just chance or 50 per cent.

With Group B the increase in the length of delay shows a regular decrease in the percentage of correct responses to 13 seconds at which delay period an inversion appears. At the 13 seconds delay is found the smallest number of trials, only 65 in number, so these were combined with the trials on which the delay was more than 13 seconds. This gave for 278 trials 61.1

per cent correct choices or a result in accord with the rest of Table XIX. If the longer delay periods are analyzed further, we find that if we combine the trials for 11 seconds and 12 seconds, for 13 seconds to 18 seconds inclusive and for all trials on which the delay was longer than 18 seconds, the number of trials and the percentage correct are 562 trials with 63.7 per cent correct, 210 trials with .63.3 per cent correct and 63 trials with 53.6 per cent correct respectively. Thus it may be seen that for this group of animals it is not until a delay period of more than 18

TABLE XIX

Showing for the 7 animals in Group B, the total number of responses, the number correct and the percentage correct at each interval of delay in seconds

INTERVAL OF DELAY	TOTAL NUMBER	NUMBER CORRECT	PER CENT CORRECT
seconds			
1	1,027	812	79.1
2	2,946	2,196	74.5
3	3,720	2,684	72.1
4	3,436	2,419	70.4
5	1,560	1,081	69.2
6	1,136	769	67.6
7	1,184	798	67.3
8	822	542	65.9
9	557	370	66.4
10	504	333	66.0
11	367	237	64.5
12	195	121	62.0
13	63	50	79.5
Over 13	215	120	55.8

seconds is reached that we find them reacting on what seems to be a purely chance basis.

From Table XX in which the results on the length of delay and the percentage of correct responses are reported for Group C, we see that the percentage of correct responses over each of the first four delay periods are about the same as for Group A except that the percentage correct for 1 second is lower, 67.8 per cent instead of 73.6 per cent. In the case of these animals as with those of groups A and B combinations of the trials for those periods of delay which involve a small number of trials show the

delay period beyond which the responses seem to be merely chance. For this group a combination of 6 seconds to 8 seconds inclusive gives 248 trials with 52.4 per cent correct responses and a combination of all delays 9 seconds or longer gives 124 trials with 50 per cent correct responses for the successive seconds.

The abrupt drop in the number of responses for the successive seconds of delay which appear for Group A at 8 seconds and for Group C at 6 seconds is not apparent for Group B. The explanation for the greater proportion of responses at the longer

TABLE XX

Showing for the 8 animals in Group C, the total number of responses, the number correct and the percentage correct at each interval of delay in seconds

INTERVAL OF DELAY	TOTAL NUMBER	NUMBER CORRECT	PER CENT CORRECT
seconds			
1	3,258	2,211	67.8
2	7,770	5,089	65.4
3	5,092	3,240	63.6
4	1,234	706	57.2
5	364	192	52.7
6	133	64	48.1
7	80	41	51.2
8	35	25	71.5
9	23	12	52.1
10	25	14	56.0
11	11	4	36.4
12	11	8	72.5
13	13	4	30.8
Over 13	41	20	48.7

delay periods for Group B than for either Group A or Group C lies in the number of times in which the animals in the various groups were trained in the more advanced stages. It cannot be due to the aggregate number of trials that were given to the three groups of animals since for Group C the aggregate was 17,750 while for Groups A and B the aggregates of the trials were 19,690 and 18,090 respectively.

It was to be expected that the percentages correct at each delay period would be higher for Group B than for either of the other groups since the animals of Group B showed better learn-

TABLE XXI

Showing for each animal the total number of responses and the percentage correct at each interval of delay in seconds

RAT	1 SECOND		2 SECONDS		3 SECONDS		4 SECONDS		5 SECONDS		6 SECONDS		7 SECONDS		8 SECONDS		9 SECONDS		10 SECONDS		11 SECONDS		12 SECONDS		13 SECONDS		OVER	
	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent
V-1	41	68	420	68	534	58	192	51	58	57	37	65	19	68	16	44	7	71	3	67			1	100	3	67	9	67
V-2	334	73	1,152	63	1,345	64	554	55	257	50	120	45	55	51	46	52	39	54	22	36	13	38	12	67	7	57	64	52
V-3	57	77	502	54	80	64	11	100	9	33	7	57	3	0	1	0	1	0	2	100			1	100	1	100	5	60
V-4	445	72	1,766	70	676	77	226	53	125	53	55	58	21	62	18	44	7	57	21	74	4	50	4	50	4	25	28	57
V-5	429	76	1,678	62	1,050	68	642	64	343	61	142	67	47	66	29	69	11	64	9	89	2	100	8	50	1	100	9	56
V-6	191	65	212	72	686	66	170	57	125	46	44	52	32	59	23	44	14	50	13	62	12	92	8	50	6	67	14	36
V-7	17	82	424	70	972	63	222	55	49	49	15	60	11	45	5	60	2	50	2	50			1	0				
V-8	43	84	439	64	971	54	245	51	37	41	13	54	5	100	7	29	2	100	2	100								
V-9	47	92	115	72	147	69	94	62	20	80	13	69	3	67	1	100	3	67	4	50								
V-10	61	87	130	70		56	33	58	11	67	2	0				0	1	0							1	100	1	0
A-1	76	90	510	78	1,220	73	668	71	295	70	398	68	461	69	206	63	135	72	173	66	147	64	57	66			25	72
A-2	78	90	585	81	524	76	630	71	587	71	338	69	228	60	176	63	113	53	63	64	27	59	25	44			36	44
A-3	149	76	361	68	323	66	122	55	53	60	52	62	43	56	18	56	13	69	9	33	2	43	3	33			11	55
A-4	492	74	978	68	1,154	69	1,080	64	185	67	55	52	27	59	11	64	3	33	4	100	2	0	2	50			6	17
A-5	120	83	333	79	363	83	871	79	411	71	286	70	422	71	408	69	301	67	254	67	183	68	107	65	46	63	136	49
A-6	43	91	75	87	31	55	5	60	3	67	1	100			2	50	1	0	1	0			1	100				
A-7	69	86	104	71	105	83	60	72	26	50	6	67	3	33			1	0										
K-1	410	66	1,484	67	1,362	60	470	57	117	50	25	40	28	43	7	86	1	0	6	50	3	0	2	100	1	0	1	0
K-2																												
K-3	653	72	1,777	64	414	64	108	55	50	54	22	55	13	33	4	100	4	50	3	67	3	67	1	100	5	20	4	50
K-4	952	68	1,245	60	213	54	53	58	39	44	16	56	9	67	3	100	5	40	5	60	2	50	4	50	2	50	13	77
K-5																												
K-6	519	75	1,619	68	1,160	62	288	57	75	59	37	51	10	60	9	56	5	80	4	75	1	100	3	67	2	50	12	50
K-7	433	63	1,186	71	699	69	273	60	76	55	26	46	17	65	11	55	7	43	5	40	2	0	1	100	2	50	8	25
K-8	291	56	459	60	244	64	42	48	7	43	7	29	3	67	1	100	1	100	2	50					1	0	2	50

ing on all of the stages and mastered more stages than did the animals of the other groups. As a consequence of meeting our criterion for so many stages and, frequently, of doing so with a relatively small number of trials the percentage must of necessity be higher than for those groups in which the animals either failed to meet our criterion or met it only after long continued training. These animals of Group B were probably reacting to the cue and not reacting on the basis of chance, at least a part of the time, with delays up to 18 seconds.

Not only from the foregoing tables (XVIII, XIX, XX) in which the animals are considered in groups, but also from Table XXI in which the number of trials and the percentage correct for each delay period in seconds is reported for each animal, is evidence found for the relationship between length of delay and the correctness of response. As long as a sufficient number of trials is represented, practically every animal shows the general tendency toward a decrease in the percentage correct with an increase in the length of delay. Additional evidence for the superiority of Group B over the other two groups appears in this table. In general, beyond the 1 second delay the animals in Group B show a higher percentage correct than do the animals of the other Groups.

C. Summary of experimental results

The following statements will serve as a summary of the experimental findings reported in the preceding pages of this section:

1. All of the animals used in the problem except two that were trained on the kinaesthetic cues learned the discrimination.

2. Only one of the animals that learned the discrimination failed to complete to the point of meeting our criterion, at least, one of the stages involving delay.

3. A summary of the number of animals completing each stage shows that our criterion was met by 23 of the 25 animals on Stage I; by 22 of the 23 on Stage II; by 14 of the 22 on Stage III; by 7 of the 12 on Stage IV; by 4 of the 7 on Stage V; by all of the 3 on Stage VI; by all of the 3 on Stage VII; by 2 of the 3 on Stage

VIII, by the one on Stage IX; by 1 of the 2 on Stage X; by none of the 1 on Stage Xa; by none of the 4 on Stage IIIA; by 1 of the 2 on Stage IVA; by none of the 1 on Stage IVAs; by 3 of the 4 on Stage VA and by none of the 3 on Stage VAs.

4. The group trained with the auditory cues showed superiority over the group trained with the visual and the kinaesthetic cues both as to the number and the difficulty of the stages completed and as to the maximal length of delay attained. The superiority held not only for the groups as a whole but for the best and the poorest animals of the groups.

5. The maximal delay period as found in the experiment was a median delay of $11\frac{1}{2}$ seconds for 80 per cent correct choices and 11 seconds for $82\frac{1}{2}$ per cent correct choices over four consecutive days for 2 animals trained with the auditory cue. The maximal delays for the animals trained with the visual cue was $6\frac{1}{2}$ seconds at $82\frac{1}{2}$ per cent correct responses over four days and for the animals trained with the kinaesthetic cues was 5 seconds with only $77\frac{1}{2}$ per cent correct choices. The inclusion of the animal trained with visual cues arranged as the presence or absence of light in the stimulus boxes shows for animals trained on vision a maximal delay of $9\frac{1}{2}$ seconds with 83 per cent correct responses over an eight day period involving 24 trials.

6. The introduction into the central pathway of auxiliary apparatus designed to impede the animal's progress sufficiently to lengthen the delay period and to disrupt differential overt bodily attitudes, if such were present, did not make the problem too difficult for mastery.

VI. DISCUSSION

In the section on Introduction it was stated that in any study of the delayed response two principal questions arise. These were stated as follows:

1. How long after the determining stimulus has been removed can the animal still make the correct response to that stimulus, and

2. What is the mechanism by means of which such a correct response is made possible when the determining stimulus is no longer present?

In this section we shall consider the relation of the experimental results obtained in the present study with those of previous studies and their bearing on the two main questions mentioned above.

A. *The period of time over which the animal can delay*

1. The maximal delay found in the present study. According to our predetermined criterion the animal gave evidence of ability to react correctly on a given stage of the problem when that animal had maintained over a four day period or for forty consecutive trials a consistently high percentage of correct responses, specifically 85 per cent correct over the four day period, with no day showing less than 80 per cent correct. The period of delay was then to be determined by the median length of the individual correct responses. The median was chosen because with the method used it was impossible to obtain delays of exactly the same length over all of the trials. Invariably there was some variation in the length of response, usually, however, over a range of only two or three seconds. In many cases if the mean length of delay instead of the median length of delay had been used, the length of delay reported would have been greater. Such would necessarily have been the case since for each stage there was a minimal delay period below which no response could be made so that any extreme case must fall above the median and would thus weight the mean unduly.

When we use the median delay over the best forty trials we find that the maximal delays were made by two animals in Group B, namely, animals A-1 and A-5. While these animals did not meet our criterion yet they so nearly attained it that considering that the stages on which they made these median lengths of delay were the most difficult, these delays are taken as our maximal delays. In the case of animal A-1 the record was made on Stage Xa with 80 per cent correct choices over the four day period with a median delay of $11\frac{1}{2}$ seconds. For the other animal, A-5, the record was made on Stage X with $82\frac{1}{2}$ per cent correct choices over the four day period with a median delay of 11 seconds. Among the animals of Group A or those trained on

the visual cue, the best record was made by Animal V-5, which made on Stage V an average of $82\frac{1}{2}$ per cent correct responses over the best four day period with a median delay of $6\frac{1}{2}$ seconds. If we include with these animals the one animal that was trained on 15 trials per day with light vs. darkness stimuli, the best record for those animals trained with visual cues, is that of Animal 15 for which the highest delay over the eight day period was $9\frac{1}{2}$ seconds with 83 per cent correct responses on Stage V. The best record made by any animal in Group C was made by K-7 on Stage IVAs on which it showed $77\frac{1}{2}$ per cent correct responses over four days with a median delay of 5 seconds.

It may be seen from the maximal delay obtained with animals from the various groups that the longest delay was found in the case of the animals of the Auditory Group. One's first impression might lead him to conclude that auditory stimulation is more effective than is visual stimulation for obtaining the delayed response in this situation. This conclusion can not be drawn from the above evidence without qualification, however. In Section IV under General Method it was suggested that the cues employed with the Auditory Group and the Visual Group were not sufficiently comparable to make possible any definite conclusions as to the most effective sense to be employed in the study of the delayed response. This possibility receives additional support from the evidence obtained from the record of Animal 15 which was trained with visual stimuli made more nearly comparable to the auditory cues by training the animal to respond to the presence or absence of a light stimulus as the Auditory Group had been trained to react to the presence or absence of the sound stimulus. With this animal it was found that a longer delay could be attained by the animal in a considerably briefer period of training than was the case with the other animals trained on vision. This was equally true whether the animal was trained with ten or fifteen trials per day and when the animal was kept continuously on one stage until it was mastered or when the animal was trained on the five stages simultaneously. On the other hand, that this animal made a record in length of delay attained more nearly similar to that of the Auditory Group

than did those animals which were trained to react to the position of the light may be a result of the different criterion used to determine mastery of the period of delay and not of the difference in the effectiveness of the cues. Furthermore, from a consideration of the evidence from the setting-up of the discrimination it may be seen that the difference between the Auditory and the Visual Groups trained on ten trials per day is slight while the latter animal, no. 15 seemed to have an advantage even for setting up the discrimination. This might indicate that our criterion was lower in the case of this one animal than in the others or it might indicate that the animal was a particularly precocious one. From the evidence at hand then, we are unable to say definitely which of the two types of stimulation is the more effective for the study of the delayed response. We can say, however, on the basis of the experimental results, that comparing the two situations as a whole, the auditory was superior to the visual situation. When we compare the results obtained with the group of animals trained to discriminate on the basis of the presence or absence of the inclined plane with the group trained on the auditory stimuli, there can be no question but that the auditory stimuli were the more effective for the study of the delayed response.

2. *Comparison of maximal delay periods obtained in the present study with those obtained by other experimenters.* As was pointed out in Section II of this study two previous workers have utilized the albino rat as subject in the delayed reaction experiment. The first of these, Hunter (6) found a maximal delay with this animal of 10 seconds, in the case of one rat when the animal was offered a possibility of the choice of one of three compartments. With the remainder of his animals he found that in the three compartment situations they were unable to delay longer than for 1 second except in the case of three animals which delayed for periods of 3 seconds, 3 seconds, and 4 seconds, respectively. When, however, he took three of the animals that could delay only 1 second in the three compartment situation and tested them on a two compartment situation he found that the animal could delay for periods up to 5 seconds. From his results it would

appear that one might expect the delays possible in the two compartment situation to be about 5 times as long as in the three compartment situation. Yarborough's (21) results from a study of the cat in a situation as nearly as possible similar to that of Hunter's found about the same relationship to exist between the period of delay obtained with two compartments and those obtained with three compartments.

The second experimenter who utilized the albino rat as subject in the delayed response situation was Ulrich (18). This experimenter using apparatus similar to Hunter's except that it was larger and had several changes made in arrangement, found that he could obtain delays of considerably greater length. As was pointed out in the section on previous work the number of trials used was so few and the percentage of correct responses obtained was so low in most cases that the present experimenter questions their reliability as measures of the animal's ability to delay. Furthermore, again as previously pointed out, it is somewhat questionable whether the proper controls were exercised in this experiment to insure that the animal was not reacting on the basis of some other cue which was present in the situation. It should be added, however, that this experimenter was not primarily interested in studying the delayed reaction in this experiment, but rather the integration of movement in learning in the albino rat.

If we now compare the maximal delays obtained by the above experimenters with the animal in the three compartment situation with the maximal delays obtained in this experiment we find that considering only the length of delay obtained our animals did not do appreciably better than did Hunter's one animal with the 10 second delay. We do find, however, that the majority of animals trained on vision in this experiment did delay longer than did the majority of Hunter's animals. If, however, we estimate what the various animals used by Hunter would have done had they been tested out on a two box arrangement, from the results which he obtained when he used only two compartments in the case of three of the animals it is quite evident that Hunter's animals would have had a better record than did the

animals in this study. This shorter delay in this study is in all probability due to several factors which we shall consider at this point.

To the present experimenter it appears that one of the chief reasons for failing to find, in this study, delays as long as those estimated for Hunter's animals in a two choice situation is that in this investigation the criterion was considerably more difficult to attain than was the criterion in Hunter's experiment. In this experiment the animal was retained on a given stage until it had given evidence of having ability to react correctly after the period of delay which was under consideration. The evidence of that ability was furnished in the present experiment by the animal's performance over a four day period in which it met the arbitrary criterion which we had set up; namely, that over the series of forty trials it made 85 per cent correct choices with not less than 80 per cent correct choices on any single day's series of ten trials. In Hunter's experiment, on the other hand, no such rigid standard was set up. In Hunter's experiment there was considerable variability in the standards as to the percentage of correct responses required of an animal before it should be advanced to a longer delay period. In addition to the percentage of correct responses he took into consideration behavior indications such as "hesitancies and waverings." While such criteria may be satisfactory to determine the maximal amount of delay of which the animal is capable; yet, when we come to the question as to the mechanism by means of which the animal is able to react correctly after delay, the more objective the evidence and the less dependent it is upon the behavior observations by the experimenter, the more satisfactory and reliable it is. Our criterion of 85 per cent correct over 40 trials is a percentage which gives us a difference from chance which is statistically reliable even though we had not further increased its reliability by requiring that this be obtained with not less than 80 per cent correct for each series of 10 trials composing the forty.

If the present criterion had been lower the animals would have advanced more rapidly from stage to stage and consequently would have covered a larger number of stages provided they had

not reached their limit of delay sooner. This is apparent from the fact that for most of the animals which failed to meet our criterion on the last stage on which they were trained, there were found four consecutive days over which the percentage correct was 75 or above. In several cases this percentage was 85 which satisfied the first part of our criterion but which did not advance the animal to the next stage since the last part was not satisfied.

Another factor in the present study which tended to decrease the length of time over which the animal could delay is found in the fact that when the animal completed the fifth stage of the training impediments were introduced into the central pathway which in addition to increasing the length of time required for the animal to run to the end of the apparatus complicated the problem so as to make the correct response more difficult. In some cases the longer time required for running through the pathway was due to an actual lengthening of path, in others it was due merely to a retardation of the animal's speed as it passed through constricted openings while in still others it was due to the necessity for making an additional series of movements such as digging before the animal could reach the point at which the choice of path to food could be made. These impediments, especially that in which the animal had to engage in an activity other than running, would tend to shorten the time over which the animal could delay successfully.

Another possible explanation of the shorter delay periods lies in the difference between this study and Hunter's study in method of presentation of the stimuli. In Hunter's study the light to which the animal was to react, was turned on for a period of 5 seconds during which the animal could "get set" for the particular response. In the present study the animal could not react to the stimulus until the release box was opened and then only for the length of time required for running from the release box to a point between the light boxes, A′ and B′, or that distance over which it passed in Stage I. If we judge this time (which could not be measured since the experimenter had to attend to the presentation if the stimulus and to the measuring of the time

of delay), from the time required to run through Stage II, this time could have been little if any, more than 1 second and many times was probably less. That such an estimate is legitimate would seem reasonable since the distance from the release box to the position where the cue was no longer present was only $13\frac{1}{2}$ inches while the distance over which the delay was measured for Stage II was 22 inches and since there was no appreciable difference in the rate of running the two parts because the animal was set to run before the opening of the release box.

In addition to these possibilities there is, of course, the possibility that, were these factors inoperative, the situation of this experiment was intrinsically of such a nature that no longer delay periods could have been attained. One would expect, however, that this was the least likely of the possibilities.

The two experimenters who have worked on the delayed reaction problem, since the work reported in this monograph was undertaken, obtained longer delays than either Hunter or the present writer. Maier's (26) set-up and method were both so radically different from that used by either Hunter or the present writer that comparison of results is almost impossible. Honzik's (27) method and apparatus are more nearly comparable to those used by Hunter as well as those used in the present study. His results differ from those obtained in this study not only in showing longer maximal delay periods but also in that with the increase in the delay periods no decrease in the percentage of correct responses appeared within the limits studied, viz., 7 seconds, 15 seconds, 30 seconds and 45 seconds.

In most reports of the experimental results on the delayed reaction, an attempt is made to relate the particular experimental findings reported with the results found on other species on the same or even in radically different situations. To the writer that procedure does not appear legitimate. Sufficient evidence has been collected with the various species to show that the period of delay is definitely a function of the method used and the particular situation involved. The only comparison that we could make would be with those studies that used the indirect method; namely those of Hunter (6), Yarborough (21), and

Walton (19). Even such comparisons are reliable only in so far as the situations in these studies are comparable. As far as the maximal length of delay is concerned, little actual difference appears between Hunter's rats, the rats in this experiment and Yarborough's cats.

B. *Evidence as to the basis for reaction after delay*

Two possibilities must be considered in regard to the basis on which the correct response may be made after a period of delay. These are (1) the maintenance of overt bodily attitudes and (2) the mediation of some non-observable intra-organic cues. Previous work with the rat has shown that the former of these is used by that animal. Hunter has presented conclusive evidence that in his study such was the case and that the bodily attitude used was gross bodily orientation. In the "Introduction" we pointed out the possibility that the particular situation in which Hunter investigated the delayed response might have been particularly conducive to the establishment of that mode of response. There too, it was indicated that the question arises as to whether in other situations the rat must use the same method of bridging the delay or whether it may show a different mode of behavior in a different situation where orientation would be more difficult.

We shall now consider the various possibilities of, and the evidence in regard to reaction by the animals in the present problem on the basis of observable, differential bodily attitudes during delay.

The first possibility to be considered is that of gross bodily orientation. If the animal used this mode of bridging the delay between the time of the disappearance of the stimulus and the making of the choice of the correct path, it would have to incline its body in the direction of the entrance to the path which would lead to reward. This would mean that in the situation used in the present experiment, the animal would either follow a diagonal path from the exit of Section B, the narrow space between the light boxes which protruded into the central alley at that point, to the exit from the central alley to the side alley or,

with its body inclined somewhat diagonally, it would run directly forward and then at the end of the central alley make the choice in accord with that inclination. Except during the first few trials after an animal had been advanced to a new stage such behavior was rarely found, and on the more advanced stages, after the second or third stages, it was never found. Animals were observed to incline the body to one side or the other, but such behavior appeared so infrequently and the behavior at the end of the alley was so inconsistent that no prediction could be made as to which side the animal would choose when it reached the end of the alley. This seems to rule out this possibility of gross bodily orientation in the present situation.

Another possibility of solution is that the animal, when it reached the end of Section B of the apparatus would make its choice of path at that point and as a consequence turn directly to the right or left as the case might be and then follow the side wall of the central alley on that side to its end and then turn into the side alley on the side of the central alley upon which it was running. This form of behavior would indicate of course, that the animal was not making a delayed response, in the sense ordinarily used, but that the choice was made as for the discrimination and the side wall was being used as a secondary cue. This behavior did appear early in the learning when an animal had been moved to a new stage. Then it sometimes happened that in the first few trials on the new stage, the animal followed a central path to the point at which it had been accustomed to making the choice of the side alley and then turned to the side according to the stimulus given. Finding the exit closed at that point the animal then continued along the wall until the exit was reached. This behavior continued only for a few trials, however, until the animal became more or less habituated to the new length of pathway. After this a definite pathway within the central alley was again selected and followed more or less regularly by the animal.

There were some animals which, when they entered the broad central alley, invariably turned to the side and followed the side wall more or less closely to the end of the alley where the

choice of path was made. For these animals, a path along one side of the central pathway became habitual and was followed irrespective of the side for which the cue was given and irrespective of the choice which the animal was to make. No evidence was found with these animals, when they were reacting so as to meet the criterion, that, at the end of the alley, they turned more frequently to the side favored in the central alley than they did to the other side. The behavior of the animals which followed such a path was indicative that the side wall was not used as a differential cue in making the choice of the path to food.

Another form of orientation that suggests itself as a possible means of bridging the delay is that of pointing the head toward the side on which the correct response must be made. Only negative evidence in regard to this appears from observation of the animal's behavior. No instance was observed in which this appeared.

There are other possibilities such as the possibility that although the animal did not orient by turning either its body or its head, yet it might have followed the side wall by means of vision. This possibility seems *a priori* rather improbable and evidence from the behavior of the animal seems to support this. It was observed at times that when the length of path in the central pathway was increased, the animal ran to the end of the previous stage and then turned toward the side. Had the animal been following the side wall by means of vision it is probable that the opening into the side alley on the side followed by vision would have been the stimulus to turn, since this was not present the animal should under those conditions continue directly forward until that was observed, but this was not the case. In a few cases the animals showed indications of having discovered that the end board had been moved by coming to an abrupt stop at the end of the old pathway and then, without making the usual turn, proceeding cautiously and hesitantly into the unfamiliar part of the apparatus until the end wall was reached. There it hesitated and finally made a choice of side alley by turning toward the opening on one side or the other.

Further indication that the above possibilities are improbable is furnished by the records of the animals on those stages involving the various impediments. This is especially noteworthy in the case of those stages on which digging through sand or passing through the S-shaped box was required. In those two situations reaction on the basis of any of the possibilities mentioned thus far seems extremely doubtful. In digging through sand, the animal showed considerable muscular activity and frequently changes of position. In passing through the S-shaped box the animal was required by the sharp turns to twist its body first in one direction and then in the other.

These experimental controls were, furthermore, of such a nature that they render highly improbable the maintenance of muscular tensions on one side or the other of the animal's body. Had such a basis been necessary for bridging the delay the stages in which these controls were used probably could not have been mastered since this muscular tension would have been lost by the animals as they twisted their bodies first one way and then the other, or as they engaged in the vigorous activity of digging. There is the further possibility that muscular tensions were present as the animal left the stimulus compartment but were lost as the animal engaged in the activity required to pass through these impediments, and then reinstated as the animal again ran forward. This seems improbable, since, if they had been strong enough for reinstatement, they would probably have been manifested in some form of overt behavior such as has been considered. Since we find no evidence for bodily attitudes which might provide a basis for the animal's ability to react correctly following a period of delay, we must conclude that the basis for the reaction is in some intra-organic cue. For the animals which failed to complete more than the simpler stages and which were able to bridge only $1\frac{1}{2}$ second or 2 second delays no indication was obtained as to their method of bridging the delay other than behavior observations which, though they are insufficient to provide any satisfactory evidence, indicate that at least, gross bodily orientation was not present. For those animals, however, that completed the stages involving impediments, es-

pecially those that include digging through sand and passing through the S-shaped box, the evidence seems to indicate that they reacted on cues other than those furnished by possible orientation.

The next question which naturally arises is concerned with the nature of the intra-organic cues on the basis of which the animal reacts after the period of delay. The present study provides no new experimental evidence of the nature of such cues nor does the present experimenter have any new contribution to offer in this respect. Various possibilities of such reaction have been suggested.

Among these possibilities we would mention that suggested by Watson (20) namely, that what may be happening is that when the stimulus is presented a definite set of arcs is released which functions serially and thus serves as the basis for responding correctly at the end of the central pathway. Our problem is of such a nature that the general situation would be favorable for reaction on such a basis. However, it is not probable that, in the part of the training involving the S-shaped box or digging through sand, such a simple basis was present for here the behavior required was so varied that a disruption of these serially connected arcs would likely have taken place. In that case they would have had to be reinstated either immediately after the animal had passed through that part of the apparatus or at least at the time when the choice was made. In such a case the question arises as to the mode of this reinstatement. Watson has said that there is no known mechanism to account for such reinstatement. Hunter, on the other hand, has pointed out that in addition to the functioning of these serially connected arcs there may be present intra-organic processes which can be rearoused without the presence of the external stimulus. Furthermore, he has pointed out (Psychological Bulletin, 1915, 12, 189–190) that there are kinaesthetic factors, having great importance for animal behavior, which can be aroused in such a manner and from that concludes that the intra-organic factors concerned in reaction after delay, where orientation has not been present, are probably kinaesthetic in nature. As previously

stated the present study furnishes no new evidence on this problem.

VII. SUMMARY AND CONCLUSIONS

On the basis of the experimental findings reported in this study, the following conclusions may be drawn:

1. The delayed reaction was investigated in a situation in which the animal was kept in motion during the period of delay instead of being confined in a small compartment as in previous investigations.

2. The maximal delay period for the albino rat obtained in this study was $11\frac{1}{2}$ seconds, the median time of delay on the correct runs when the animal made 80 per cent correct responses over 40 consecutive trials.

3. The maximal delay period here obtained was shorter than that estimated for Hunter's rats had his best animals been tested with two choices instead of three. Possible explanations of this fact may be made on the basis of the higher standard of performance required by our criterion of learning; the great complexity of our problem in the higher stages, and the differences in the method of presenting the stimuli.

4. Reaction in the situation present in this study was found, at least in some cases, not to be on the basis of overt bodily attitudes. Evidence for this was found in the animal's general behavior and in his ability to react correctly after such attitudes, had they existed, must have been disrupted.

5. The experimental evidence presented indicates that some of the animals reacted on the basis of some intra-organic cues, the nature of which cannot be determined from the data collected in this study.

6. Visual, auditory and kinaesthetic stimuli were all effective in producing correct response after delay.

7. No definite conclusion can be drawn as to the relative effectiveness of auditory and visual stimulation for producing correct response after delay.

8. Both visual and auditory stimulation as used were more effective for obtaining correct reaction after delay than was the

particular type of kinaesthetic stimulation which was made use of in this study.   The superiority of the two former types of stimulation was shown by (a) the best stages reached in the experiment; (b) the number of trials necessary to attain the highest stage; (c) the delay period actually attained.

9. An inverse relationship was found between the percentage of correct responses and the length of delay as is shown by a comparison of the percentages of correct responses for the various delay periods irrespective of the length or condition of the pathway upon which they were made.

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COMPARATIVE PSYCHOLOGY MONOGRAPHS

YALE LABORATORIES OF COMPARATIVE PSYCHOBIOLOGY

BY

ROBERT M. YERKES

Yale University

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PLAN AND PROVISION FOR SCIENTIFIC USE OF ANTHROPOID APES

As a statement of definite achievement this report supplements earlier proposals and programs of research.[1]   An announcement of achievement is the more gratifying because it is the outcome of nearly two decades of persistent effort to advance psychobiological research.   There has now been attained excellent constructional provision for use of the anthropoid apes and, as indicated, other animals, in concerted attack on the primary problems of psychobiology.   In our present quest for knowledge of the nature and relations of consciousness and behavior there are proximate goals in such varied assemblages of biological problem as the ecological, the morphological, and the sociological.

The Laboratories of Comparative Psychobiology of Yale University are constituted by three geographically separate centers of activity: (1) New laboratory and primate quarters in conjunction with the laboratories of neurophysiology of the Yale School of Medicine, New Haven; (2) an anthropoid breeding and observation station at Orange Park, Florida; and (3) arrangements for coöperative use of temporary and permanent scientific stations in Africa where the chimpanzee, gorilla, and other primates may be studied in their native habitat.   Administratively these laboratories together constitute a section of the Department of Physiology under the chairmanship of Doctor John F. Fulton. It is hoped and believed that this intimate association of psycho-

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[1] Yerkes, Robert M.   Provision for the study of monkeys and apes.   *Science*, N.S., 1916, 43: 231–234.
—.   A program of anthropoid research.   *Amer. J. Psychol.*, 1927, 39: 181–199.

biology with physiology will help to bridge the gap between psychology and neurophysiology by facilitating contacts and cooperation among investigators.

ESTABLISHMENT OF PRIMATE LABORATORY OF THE INSTITUTE OF PSYCHOLOGY, NEW HAVEN

Following the establishment of the Institute of Psychology, Yale University, in 1924, the writer obtained funds to test the feasibility of keeping and using chimpanzees as subjects of biological experiment in New Haven. The Primate Laboratory was organized in 1925 in temporary quarters, and four young chimpanzees were immediately procured for use as subjects. These animals became increasingly gentle and docile and also improved markedly in physical condition as they were used in the laboratory. They served as subjects in at least a score of studies, which included bodily and behavioral growth and development, nutritional requirements, modes of behavioral adaptation, aspects of social relations and of reproduction. In the bibliographic list at the end of this paper appear reports of our practical experience in maintaining and observing the apes and of the study of specific problems.

Responsibility for the welfare of the apes and the labor of caring for them were shared by three staff members. The writer's long-standing interest, initiative, and administrative activities were ably supported by Doctor Harold C. Bingham, who during the five years of his connection with Yale University devoted himself whole-heartedly and successfully to the interests of the undertaking, and by Doctor Otto L. Tinklepaugh, whose specialization of interest on problems of nutrition, health, and reproduction has advantageously supplemented the work of his senior colleagues. It is possible for the writer as Director of the Primate Laboratory to recognize the services of these and more transient members of the laboratory, but it is quite impossible to make adequate acknowledgment. To them certainly the early and continuing success of the enterprise is very largely due.

That the Primate Laboratory during the six years of its existence was located in a barn by no means lessens our satisfaction in

convincing demonstration of the practicability and scientific importance of attempting to utilize the chimpanzee as material for solution of varied biological problems. Aside from our time and the acquisition of gray hairs, this demonstration cost little! Retrospection in this connection suggests the possibility that research values may often be inversely related to costs. This particular bit of pioneering and ground-breaking in psychobiology, which was conducted in extreme simplicity of material circumstance and on a relatively low scale of living, speedily made its way to a happy conclusion. True, we glory in the fact now more than we enjoyed the circumstances of work erstwhile.

FOUNDING OF THE ANTHROPOID EXPERIMENT STATION AT ORANGE PARK, FLORIDA

During the fourth year of the existence of the Primate Laboratory, the writer presented to President Angell of the University a plan for continuance, extension, and improvement of provision for systematic research with the anthropoid apes. This plan was based on convincing demonstration of the practicability of such work and on definite exhibits of organizational and research achievement. Because of the novelty, excessive risks, and prospective costliness of the undertaking, the university administration sought the advice and recommendations of a specially constituted committee, which consisted of Messrs. James R. Angell, Edwin G. Conklin, Milton J. Greenman, John C. Merriam, Theobald Smith, H. Gideon Wells, and Clark Wissler. The writer served as secretary of the group. Following inquiry into the facts and appraisal of the merits and probable returns of the project, the committee rendered favorable recommendation. Thereupon Yale University successfully sought funds to carry the plan into effect. The members of the original committee most generously agreed to serve as Advisory Board of the prospective Anthropoid Experiment Station.

The establishment in 1930 of a suitable station for breeding and observing apes was the first step taken by the University in extension and improvement of provision for anthropoid research. For this purpose an eminently suitable tract of nearly two hun-

dred acres of wooded land in Clay County, Florida, about fifteen miles from the city of Jacksonville, was purchased. Under the writer's general supervision, architectural and engineering plans for station buildings were prepared by Messrs. Richard A. Kimball and Ellery S. Husted of the office of James Gamble Rogers, New York City. The plans were executed by Mr. Arthur F. Perry, Jr., engineer-contractor of Jacksonville. The buildings have proved highly satisfactory in appearance and utility.

The initial unit of the station plant was designed for a colony of approximately twenty-five chimpanzees or other great apes. It consists of (1) three main buildings—Laboratory, Service Building, and Animal Quarters Building—which are connected by an enclosed corridor; (2) a roofless enclosure surrounded by a fourteen-foot iron fence; (3) four outlying smaller buildings— Hospital or Isolation Building, Garage, General Storage Building, Pump House and electrical distribution center; and (4) a residence for the superintendent of the plant.

The laboratory is a plain, inexpensive, two-story structure of hollow tile and stucco, which is reasonably well adapted to requirements and convenient and comfortable as a working place. It possesses no noteworthy technical features of design or construction, with the exception of a room built within a room (see plate 7) to facilitate varied measurements of behavioral traits. The subject works in the inner room, the walls of which are built of heavy wire netting; the experimenter is located in the outer room. This arrangement provides exceptional adaptability without essential changes in laboratory space. Standardized apparatus may be placed either within or without the inner or response chamber, while grilles of suitable dimensions for the animals to work through may be placed at any point in the wall of the inner room, and appropriate recording apparatus may be located in the outer room or elsewhere as desirable. The arrangement is much more convenient and flexible than connecting rooms, so commonly employed where it is necessary completely to isolate subject from observer.

The Service Building is a one-story structure with basement. It is built of hollow tile and stucco. The basement provides

adequately for furnaces and fuel storage.   On the ground floor of the building are a workshop with motor-driven saw, planer, grinder, buffer, hack saw, drill, and screw-cutting lathe, a kitchen with necessary cooking and cleaning equipment, and a food storage and refrigeration room.   This structure, located between the Laboratory and the Animal Quarters and connected with each by corridor, also offers convenient approach to the animal cages and to all outlying buildings, including the residence of the superintendent.

The Quarters Building cost the writer far more labor of planning than all the other buildings combined.   It is unique in design and in its special mechanical features.[2]   Built of concrete and steel, the Quarters is wholly fireproof.   There are two stories, in addition to basement which houses ducts and fan for ventilating and heating systems.   The ground floor provides five rooms on each side of a central corridor.   These small rooms, with the exception of two which are used for examining and service purposes, serve as animal living rooms and each communicates with an out-of-door cage.   A portion of each cage is protected from sun and rain by a concrete roof which extends from the Quarters Building for a distance of eight feet.   Neither in corridor nor living rooms are there structures which the apes can readily get out of order or destroy.   All gates and doors either are mounted on rollers or move vertically.   They are equipped with special automatic locks.   The corridor and living rooms are lighted from above, naturally or artificially as desired.   The second story of the building houses appliances for control of illumination, ventilation, heat, and also for manipulation of shutters and doors.   Construction throughout was planned to withstand the destructive capacity of the adult male chimpanzee.

With the exception of the superintendent's residence and the garage, the aforesaid buildings are all located within an eight-acre enclosure which is surrounded by a seven-foot fence of heavily galvanized chain-link netting.

[2] Grateful acknowledgment is due the engineers of Anchor Post Fence Company for their assistance in designing and for installation of cages, sliding and rolling doors, grilles, and a variety of locking devices.

The construction of the Anthropoid Experiment Station was completed in June, 1930, and immediately the four chimpanzees which had been used for five years in the Primate Laboratory in New Haven were transferred to Florida to constitute the nucleus of a breeding colony. Since then several young individuals have been added to the colony by gift of the Pasteur Institute Laboratories at Kindia, French Guinea, thanks to the generous coöperation of Doctor Robert Wilbert, Director of the Laboratories, and of Doctor M. Delorme, Assistant Director. These animals were brought from Africa to the Yale laboratories by Doctor Henry W. Nissen, staff member, following a period which he profitably devoted to initiating naturalistic study of the chimpanzee in the vicinity of Kindia.[3] Yet other specimens were added by purchase, but by far the most precious additions, by reason of their known life history for a considerable period and known age in several instances, came as the gift of ten individuals from Mr. Pierre S. Abreu of Havana, Cuba. This gift alone advances the work of the Station by at least two years, since in addition to three young females of known birth dates, it provides several sexually mature females, accustomed to captivity and well acclimated.

Effort at the Station has been concentrated thus far on the development of an excellent colony of breeding animals and the achievement of nutritional, hygienic, and social conditions favorable to reproduction and the rearing of young. As practicable, studies of aspects of growth, development, physiological process, and psychobiological traits are being pursued. Progress in all directions has surpassed reasonable expectations. The first chimpanzee birth occurred at the Anthropoid Experiment Station in September, 1930. In 1931 there were two births.

The number of investigators at the Station thus far has varied from two to four. The writer and Doctor Otto L. Tinklepaugh have been responsible primarily for direction and administration. They have been competently assisted by Doctors Carlyle F. Jacobsen and Joseph G. Yoshioka.

Publications from the Station, as also from the New Haven

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[3] See Nissen report as listed on p. 22.

laboratory, will appear in appropriate scientific serials, and especially in *Comparative Psychology Monographs* and the *Journal of Comparative Psychology*.   Several reports are now in press or in preparation.

According to intent, the Anthropoid Experiment Station will serve both as observational establishment and as a source of relatively standardized and definitely described chimpanzee subjects for experimental use.   It cannot at present be predicted whether the supply of animals will prove more than adequate for the needs of the Yale University laboratories.   It is hoped, however, that it may and that other institutions and investigators may thus be assisted.   Naturally all scientific aspects of chimpanzee reproduction, from the morphological to the sociological, are being studied at the Station, but simultaneously and as practicable other interests are being developed and other problems attacked.

PERMANENT HEADQUARTERS LABORATORY IN NEW HAVEN

While arrangements were being made for the establishment of an anthropoid breeding station, plans were prepared for a new permanent laboratory in New Haven.   Construction of this laboratory, as portion of a wing of the Sterling Hall of Medicine, was completed during the summer of 1931, and in August equipment and chimpanzee colony were moved from the old Primate Laboratory to the new quarters.   With abandonment of the laboratory which had originally been organized as part of the Institute of Psychology, administrative affiliation was transferred from the Department of Psychology to that of Physiology and the earlier designation of the laboratory was changed to "Laboratories of Comparative Psychobiology."   As previously stated, this name is used as inclusive of the New Haven academic headquarters laboratory, the Anthropoid Experiment Station in Florida, and any other outlying Yale organization or activity financed and operated conjointly therewith.

The New Haven laboratory, as planned by the writer and his associates Doctors Nissen and Tinklepaugh, consists of (1) especially designed quarters for chimpanzees or other primates and

for their care and observation, constructed as a penthouse (fourth) floor of the building; (2) the equivalent of approximately thirty-six units (sixteen by twelve feet) on the uppermost floor (third) of building (see plate 8, X); and (3) two unit-rooms on ground floor. The third floor space provides administrative offices, studies, conference room, shop for use of investigators, battery room, instrument room, dark rooms, sound-proof room, suite of three rooms for colonies of small animals, two large rooms containing portable observation booths, and numerous large and small experiment rooms for use in accordance with need. Much of the laboratory space, by reason of unit construction, is at once convenient and highly adaptable. Special features of design are most numerous in the anthropoid quarters, plate 9. Here separate suites are provided which consist of living room, with heated, humidified air as required, and continuously accessible out-of-door cage; there are also a kitchen for food preparation, a food storage room, corridor dining rooms for animals, janitor's service and shower room, and a special room for physical and physiological measurements and for photographic recording, projection, and storage of photographic materials and equipment. Two portable observation booths, designed by the writer, are available for use in experiment rooms as required. Each booth is approximately 2.4 meters high by 2 by 2 meters, outside dimensions, and provided with observation windows in two adjacent walls and with equipment for ventilation and illumination.

In the organization and administration of this laboratory the writer has been assisted invaluably by Doctor Henry W. Nissen, who in turn has had the help of Messrs. James H. Elder and Kenneth W. Spence. Mrs. Helen S. Morford, as Secretary of the Laboratories and editor of records, has contributed notably to the system of recording and to the creation and maintenance of excellent informational files.

The New Haven laboratory is designed and equipped for psychobiological research. Its resources are available to graduate students in the biological and social sciences, to students of medicine, and to visiting fellows and other competent investigators. The chimpanzee and other animals are used as experimental sub-

jects in the investigation of a wide range of problems. Institutional resources and needs, individual interest, and practicability alone limit the scope of inquiry, for the Laboratories of Comparative Psychobiology are as hospitable to the techniques, methods, needs, and opportunities of the social science divisions and aspects of biology as of the morphological and the physiological; as favorable to studies of the pathological subject as of the normal. In general, conventional boundaries of the sciences are considered and respected merely as indicators of helpful specialization. Already in the initial year of the laboratory's activities, arrangement has been made for coöperative investigations by members of our staff and those of the Laboratories of Physiology, Pathology, Radiology, and Anatomy. Thus by interlaboratory and interdepartmental conference and coöperation, risks of misunderstanding and wasted endeavor are minimized and the advantages of specialization of knowledge, insight, and technical skill are guaranteed the investigator.

The Primate Laboratory of 1925 to 1931, instead of being abandoned in favor of the Anthropoid Experiment Station in Florida, as many have assumed, has been transformed into the academic headquarters of the Laboratories of Comparative Psychobiology of Yale University, an organization whose primary reason for existence is extension of knowledge of psychobiological phenomena. It is the basic idea in the organization of these laboratories to use anthropoid subjects as extensively as practicable, but also intensively, for the fruitful increase of knowledge of life and as means of approaching the solution of various human problems for which we may not freely and effectively use ourselves as materials of observation and experiment.

Members of the laboratories are migratory, and animal subjects, equipment, problems, and interests are transferred on occasion as circumstances require. Actually, the Florida station is merely an extension or outpost of the academic laboratory in New Haven. This is true also, as later paragraphs will make clear, of field stations, expeditions, and naturalistic activities which enable the staff to advance knowledge of the wild chimpanzee.

Although organized and conducted primarily for research, the

Laboratories of Comparative Psychobiology offer opportunity for professional training in psychobiology (inclusive of comparative and genetic psychology as now used) and for self-discovery and self-development through creative activity in this particular field of inquiry. A seminar for eligible student applicants and members of the laboratories is offered annually by members of the staff, and research opportunity also is available for those who while working toward professional competence desire an academic degree or other certificate in psychology, physiology, or medicine.[4]

PROVISION FOR NATURALISTIC STUDY OF ANTHROPOID APES

The academic laboratory requires supplementation in yet another direction than by establishment of a subtropical breeding station to supply animals of known age, ancestry, and life history, for a background of reliable knowledge concerning the wild free individuals is wholly essential for satisfactory use of captive specimens and for safe interpretation of results which they may yield. Evidently then our purpose to render the chimpanzee preëminently useful as an experimental subject can be achieved only when varied studies are conducted in nature as well as in the laboratory. Accurate knowledge of natural and typical physical and behavioral growth and development, of characteristic diseases and pathology, of innate and acquired behavioral adaptations, of social relations and reproductive processes, would appear to be especially important for safe comparison and correct interpretation of observations. That much of the information suggested cannot be obtained in laboratories within the temperate zone seems almost certain, for the environment of captivity may bring about modifications which only comparable studies of the wild animal will reveal. Therefore, it has seemed essential to formulate a program and plan for the study of the chimpanzee in its native habitat and, as feasible, to proceed with such work.

It is not necessary to establish observational stations and to

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[4] Information concerning these courses, opportunities for research, or appointment to scholarship, fellowship, or assistantship in the laboratories may be obtained by addressing the Director, Laboratories of Comparative Psychobiology, 333 Cedar Street, New Haven, Conn.

build special laboratories for naturalistic study of the chimpanzee, because suitable or adaptable establishments, which we may hope to use coöperatively, already are available in French, Belgian, and British possessions on the African Continent.

Our staff has already tested the practicability of international coöperation in the naturalistic study of the African great apes. In 1929 Doctor Harold C. Bingham, with the support of Yale University and the Carnegie Institution of Washington, proceeded to the Albert National Park in the Belgian Congo and undertook observation of the behavioral traits and environmental relations of the mountain gorilla, *Gorilla beringei*. This was done with the generous aid of the Belgian authorities, and particularly that of Doctor J. M. Derscheid. Doctor Bingham's report of his observations has ecological, psychological, and sociological value.[5] Developments in the Belgian Congo justify the hope and expectation that from a scientific station which is now being planned it may be possible to carry on certain studies of the chimpanzee as well as of the mountain gorilla. We have not dared to expect continuity of such research through the efforts of members of our staff, but, as funds can be secured and members of the staff temporarily spared, it is planned to promote this aspect of our inclusive program of study and scientific utilization of the great apes.

Simultaneously with Doctor Bingham's work in the Belgian Congo, Doctor Henry W. Nissen initiated naturalistic study of the chimpanzee in French Guinea. His work was done in coöperation with the staff of the Pasteur Institute Laboratories at Kindia, and with the immediate assistance of Doctor M. Delorme, who was then resident as Assistant Director of the Laboratories. The Kindia station, popularly designated as Pastoria, served Doctor Nissen as base of operations and his field work was done within a radius of forty miles of the town of Kindia, in a region frequented by bands of chimpanzees. A professionally valuable report of the results of Doctor Nissen's field study of the chimpanzee, as indicated in the list of publications on page 22, is already available.

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[5] Manuscript ready for publication.

The possibility of combining in a single institution reasonably satisfactory conditions and facilities for study of the wild ape, for breeding and rearing experimental subjects, and for conduct of research on varied biological problems, was not neglected in the formulation of plans for our laboratories. Assuredly it might have been done in certain parts of Africa, but the disadvantages over such distribution of provisions as is represented in the Yale Laboratories of Comparative Psychobiology seemed prohibitive. In addition to the relatively high cost of research in time and human effort, there appeared the threat of unreasonable sacrifice of social and professional contacts and material facilities through isolation, the physical risks of tropical climate, and the probability of relatively low and possibly progressively lessening efficiency of investigators with continued residence in the tropical habitat of the great apes.

Pertinent to these considerations is the fact that if ever there has existed almost ideal transition from natural to artificial conditions of anthropoid life it appeared in the Abreu primate colony in Havana, Cuba,[6] where for upward of thirty years Mrs. Rosalia Abreu maintained, bred, and reared great apes and many other types of primate. It was her hobby. Yet even her tropical estate, Quinta Palatino, failed to simulate closely the natural habitat of chimpanzee or gorilla.[7]

This structural and functional account of the Laboratories of Comparative Psychobiology may thus be summarized. The academic headquarters laboratory in New Haven is at once center for administration and organization of program and locus for intensive specialized individual and coöperative attack on definite problems; the Anthropoid Experiment Station supplies apes for experimental use and also affords opportunity for studies in the physiology, psychobiology, and sociology of reproduction; and finally, field and habitat studies of the great apes in the tropics supplement the research of each of the laboratories.

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[6] For description of this remarkable colony see volume by Robert M. Yerkes, "Almost Human," New York, 1925.

[7] With the death of Mrs. Abreu in 1930 this primate colony was abandoned.

PRINCIPLES OF ORGANIZATION AND OPERATION OF LABORATORIES IN RELATION TO OBJECTIVES

It remains to present briefly certain important principles of organization and operation in relation to objectives and to significant characteristics of the research situation in which we are endeavoring to labor constructively.

The laboratories constitute a unitary establishment, with a single staff whose members work wherever institutional and personal needs dictate: now at the University, now in Florida, and again in Africa or other primate habitat.

The following considerations are of extraordinary importance. Practical information about care of the animals, and especially about diet, hygienic precautions, diagnosis and treatment of physical ills, attention during pregnancy and period of nursing, must be relatively independent of the accidents of individual interest or period of service on the staff, in order that such experience may be cumulative and its value steadily increase. Our animals are relatively long-lived, serving possibly for decades; our colony is a continuing social group whose members, if maximal efficiency and reliability of work are to be achieved, must be treated nutritionally, hygienically, medically, and surgically, not as one would treat the members of the customary laboratory colony of cats, rabbits, guinea pigs, or rats, but more nearly in accordance with human requirements. Whereas the ordinary experimental animal is of relatively slight value, one of our chimpanzee subjects, by reason of years of service in varied experimental situations and as breeder, may be highly valuable. For these reasons, and others which need not be suggested, it would be wholly inexcusable to conduct this laboratory's colony of apes and its program of observational work primarily on the basis of individual interests, desires, prejudices, or whims. Instead, over-individual institutional interests should dominate.

In order to safeguard coöperative endeavor and to minimize the risks of individualism, the following Code of Regulations was prepared and adopted by the staff in 1930:

Code of Regulations

Coöperation. Although individual initiative, independence, and originality are primary conditions of success, the nature of the program of research of these laboratories is such that teamwork and an exceptional degree of coöperativeness, subordination of personal interests, and consistent efforts to contribute to the furtherance of the general program of research, and especially to the necessary routine work of the establishment, are absolutely indispensable. Obviously, breadth of mind, sympathetic understanding of the laboratories' objectives, and disinterested coöperation must be expected of every investigator.

Each member of the laboratories, by arrangement with the Director, will participate in responsibility for the care of animal subjects, will contribute to the systematic records of observation, and will endeavor to maintain and improve the morale, efficiency, and spirit of worth-while endeavor in the laboratories. For other than transient success and promotion in responsibility, integrity is no less indispensable than unselfishness.

Rights and privileges. All photographic and other forms of record which are of general and continuing interest shall be the property of the laboratories, save as otherwise agreed between Investigator and Director. They shall bear the name or identifying mark of the investigator who is responsible for them and he shall have priority in their use. If it should be desired to use them otherwise during the connection of said investigator with the laboratories, his approval shall be secured and suitable acknowledgment shall be made.

Such continuing and cumulatively valuable records as the Director's Log, individual daybooks, data of routine periodical anthropometric, physiological, and behavioral measurements shall be filed in the office of administration and ordinarily shall not be removed therefrom.

Records of specific investigations usually shall remain in the keeping of the responsible investigator until the study has been completed and report published.

Publication and publicity. All publications from the laboratories, or based upon observations made therein, shall be approved in manuscript by the Director and shall carry suitable institutional heading, together with proper acknowledgments to other individuals and institutions. Except as specifically arranged, the laboratories shall assume no responsibility for cost of publication or for supply of reprints.

Publicity is controlled by the University. All matters of news print, magazine, or advertising publicity shall be referred to the Director.

Treatment of animals.   The rules regarding treatment of experimental animals adopted by the American Medical Association and the American Psychological Association, and posted conspicuously for the information of investigators, are in force in these laboratories.

To insure so far as practicable accumulation of valuable experience and varied materials of observation, including standardized measurements, a system of recording has been devised which includes three types of systematic records: (1) Laboratory or Director's Log, a record of important events which make laboratory history; (2) Individual Daybook or diary records, an account of the life history of each chimpanzee subject; and (3) supplemental photographic and other records of experiments, apparatus, and subjects.

There follow the specific instructions available to members of the laboratories for the keeping of Laboratory Log and Individual Daybook records.

The Director's Log

Matters of general and exceptional importance to the laboratories shall be entered in the Log by the Director or his representative.

Appropriate subjects for record are, for example: Arrival or departure, birth or death, of animal subjects; accident, illness, sexual maturation; other than minor or transient changes in diet, housing conditions, opportunities for play, social grouping or isolation of individuals; initiation of new investigations, especially notable observations, discoveries, lectures, publications, or other announcements from the laboratories; arrival or departure of staff or student investigators; visits of distinguished investigators or other persons professionally concerned with the work of the establishment.   The volume may well serve as visitors' book and bear the signature of such individuals.

The Log, as summary history of the laboratories, shall be discriminatingly and carefully written.   Only exceptionally should it duplicate individual daybook and other special records.

Instructions for daybook recording and use of record forms

The system of recording described below has been adopted in order to achieve accuracy, uniformity, and continuity of records of the life history of our chimpanzee and other primate subjects.   Certainty

and promptitude of recording of many miscellaneous observations, as well as of many types of measurement, are essential to satisfactory progress. Practical experience concerning the care, handling, treatment, and use of our subjects should be accumulated in such form as to be readily available for emergency use. The daybook and special form records will constitute ultimately a consecutive and reasonably complete account of the history of each individual throughout its existence in our laboratories.

An unusual degree of coöperation and teamwork is essential to the achievement of our principal objectives. Highly developed self-interest is sure to be unfavorable to progress. The motto of our establishment might well be, "Each for all and all for each."

In addition to the daybook record there are used regularly as conveniences for reporting observations the following forms, to which from time to time others are to be added: Dental Record, Summary Individual Record, Body Measurement Record.

Detailed instructions are necessary only in case of daybook reporting. Under this head, for the guidance of investigators, definite answers are offered to the following questions: Who should prepare the records? What should be recorded? How should the record be made?

Relative to the question of responsibility for recording, the following instructions obtain. The person in charge of the colony, particular individual, or individuals, is held primarily responsible for entries. In supplementation of his entries, any observer who makes special examination or any experimental or other use of an animal shall, on the date of such use, report same in accordance with instructions below. Finally, any observer who notes some unusual bit of behavior or condition of an individual should immediately record the same despite the possibility of duplication. With reference to the latter, no individual should hesitate to record an observation because of chance of duplication, since the initial records are typed for permanent use and in the copying duplication can be eliminated.

As to what should be recorded, the guiding statement is, whatever data concerning the environmental conditions, physical status, behavior, social relations, etc., of the individual, are likely to be of interest and value in the description of the subject and in connection with its use in special experimental inquiries. This general statement may be usefully supplemented by a classification of data. Although the particular classification suggested is necessarily tentative and it is highly probable that it will be modified and supplemented from time to time, it has proved

useful to recording observers as a general guide and as suggesting leading captions for entries.

Classification and partial list of happenings which should commonly be made subject of immediate report when observed or when change occurs

Habitat—where and how housed, indoor or outdoor life.

Environment—conditions and changes in same. Characteristics of weather, especially temperature, humidity, storms.

Health—general condition, activity, playfulness; indications of indisposition; development, symptoms, treatment, etc., of diseases or disabilities; accidents, operations, etc.

Diet and feeding—exact description of diet, both qualitative and quantitative, and of changes in same; appetite, preferences, nutritional condition; habits of eating and drinking, etc., etc.

Examinations and measurements—medical, dental, anthropometric, physiological. Daybook entries should supplement but not duplicate the observations recorded on special record sheets.

Work—designation of type of experiment used in or of observations systematically made; any miscellaneous use of animal, experimentally or otherwise.

Social behavior and relations—as for example, exhibitional, decorative, coöperative, sex, imitative, etc.

General behavior—appearing, developing, or changing behavior, as in creeping, walking, dancing, handclapping, mannerisms, instrumentation, invention, learning, adaptation, etc.

Emotional expressions—appearing and developing emotional patterns, designation and description.

Reproductive behavior—sex maturation, masturbation, mating, gestation, parturition, etc.

It will facilitate use of permanent daybook records if so far as practicable observations are recorded under such classificatory captions as have been suggested. The recorder must use practical judgment and profit by experience in deciding what to report. Especially at first, the safe rule is to record too much and in too great detail instead of the reverse.

The record forms, of which several are mentioned in Instructions for Daybook Recording, are conveniences for the observer in

uniformly reporting results of systems of measurement. A system of bodily (anthropometric) measurements has been agreed upon and is in use. A system of physiological measurements, and likewise of behavioral measurements, is in preparation. It is intended that these systems, as selected batteries of specific measurements, shall supply material for serviceable description of the individual primate in terms of bodily, physiological, and behavioral characters and capacities.

To repeat, the purpose of the systematic recording as described above is two-fold: to traditionalize practical experience so that it shall be not only lasting but also cumulative, and to render immediately available in the permanent files of the laboratories complete account of the life history of each individual and data of observation which fully characterize the individual at various stages of development with respect to physique, physiological characteristics, and behavioral traits.

Once the ideal of completeness, thoroughness, and accuracy which we are striving for has been achieved, we shall proceed, far more definitely than has hitherto been the case in anthropoid research, from knowledge to knowledge. For our subjects, instead of being accepted on faith, will be intimately and reliably known in their ancestry as well as in their post-natal existence. When individuals are to be mated, it then may be done in the light of pertinent knowledge of ancestry and ancestral traits, age, individual experience, physique, physiological and mental characteristics. Still more important is the fact that when the program of the laboratories has been carried into effect completely, these several assemblages of trait will be described, for purposes of ready comparison, in terms of measurement as contrasted with individual opinion or surmise. The phrase 'standardization of materials of research' indicates the direction of our endeavor; lessening of the wastefulness of individualistic, piecemeal, sporadic work is a wayside objective.

If the regulations as formulated, or the description of objectives, suggest mistrust of individual initiative or attempt to discourage it, there is serious misunderstanding. Initiative is essential to progress in discovery and invention, and it is prized

accordingly in these laboratories and encouraged in so far as it is consistent with group responsibility and service.   Necessarily and justly in such an establishment as this, each member of the group must carry his burden of routine chores.   They are happily constituted who, without sacrificing creative effort and productivity, can transform routine into interesting and meaningful experience and thus capitalize for their self-development and the increase of knowledge what to the average individual is irksome physical labor.

At the moment, laboratory endeavor is directed toward accumulation of an essential body of reliable information concerning the chimpanzee.   That many lines of inquiry, seemingly unrelated, are being followed is natural and from our point of view desirable. Presently it is hoped that we shall have such ample knowledge of the characteristics of our ape subjects that they may be used in attack on specific problems with a degree of directness, definiteness, and intelligent planning and foresight which otherwise would be unattainable and which is regrettably uncommon in biological research.

At the date of writing, November 10, 1931, inquiries are in progress in one or other of our laboratories in the following general fields: studies of diet and nutrition; hygiene, diseases, therapy; parasitology; physical, and especially skeletal, growth and development; physiological processes, characteristics, and variations; psychobiological development; appearance and maturation of primarily innate activities; aspects of behavioral adaptivity or acquisition of habits; emotional and other aspects of affectivity and its expressions; analyses of perceptual processes; survey of auditory and visual modes of sense and response; response with insight; response with foresight; language and symbolism; physiology and psychobiology of reproduction; relations of parent and young; varied problems of ontogenesis; social relations, institutions, and patterns of response; neurological relations of acquired behavioral adaptations; skin manipulation and treatment as individual-social pattern of response; strength and neuromuscular efficiency; phylogenesis, as suggested by structural and psychobiological facts.

In an earlier publication,[8] "A program of anthropoid research," certain assemblages of psychobiological problem were listed as offering peculiarly significant opportunity for utilization of anthropoid subjects.   We quote:

By enumeration of thirteen major assemblages of problems or fields of inquiry, each of which is briefly characterized and exemplified by selected problems, a program of anthropoid research has been schematically presented.   The grouping of problems is practical rather than logical and the list which follows makes no pretense at completeness.   (1) Action system; (2) receptivity and sensibility; (3) expressivity and affectivity; (4) temporal relations of psychobiological phenomena; (5) perceptions, configurations, coördinations; (6) mnemonic processes; (7) ideation, insight, thought, generalization; (8) motivation; (9) adaptivity and education; (10) language and symbolism; (11) norms and differences: individual, sex, age, species, environmental; (12) social relations; and (13) conduct and morality.

Of the above problem groups, all except (4) temporal relations of psychobiological phenomena and (13) conduct and morality, are represented by present activities.

In many directions the investigative interests enumerated constitute pioneering; in others they are definitely a matter of bringing the techniques of other sciences to bear on what are primarily psychobiological problems.   Clearly interest is limited to no single division of science, and quite as clearly no single method or point of view would suffice to guarantee progress.   The dominant purpose evidenced is to utilize the chimpanzee as scientific subject with maximal effectiveness for extension of knowledge of life and mind, and ultimately for coöperative attack on the problem of consciousness.

Achievement of the excellent provisions for research offered by these laboratories, of a serviceable body of practical experience in maintaining individuals and colony, and of notable progress in the accumulation of valuable information for attack on specific psychobiological problems, is due to the self-subordinating coöperative efforts of every member of the laboratories and staff.

[8] *Amer. J. Psychol.*, 1927, 39, p. 198.

Acknowledgment of conspicuous services has been made by mention of certain individuals. It remains to give credit generally and inclusively by presenting the names of those who as appointees or visiting investigators furthered the work of the former Primate Laboratory, and to present the membership of the Laboratories of Comparative Psychobiology for the academic year 1931–1932.

Investigators prior to present organization. Harold C. Bingham, Research Associate; Donald K. Adams, Research Assistant and Sterling Fellow; Margaret Child Lewis, Research Assistant; Chauncey M. Louttit, Research Assistant; Morgan Upton, Research Assistant; Joseph A. Gengerelli, National Research Council Fellow in Psychology; Eilhard von Domarus, Fellow of the Notgemeinschaft der Deutschen Wissenschaft.

Staff and visiting investigators 1931–1932

Name	*Title*	*Location*
Robert M. Yerkes	Director (Professor Psychobiology)	New Haven and Orange Park
Otto L. Tinklepaugh	Research Associate	New Haven and Orange Park
Henry W. Nissen	Research Associate	New Haven
Carlyle F. Jacobsen	Research Assistant	New Haven
Joseph G. Yoshioka	Research Assistant	Orange Park
James H. Elder	Assistant	New Haven
Kenneth W. Spence	Assistant	New Haven
Louis W. Gellermann	National Research Council Fellow	New Haven
C. Ray Carpenter	National Research Council Fellow	New Haven and Orange Park
Winthrop N. Kellogg	Social Science Research Fellow	Orange Park
William C. Atwater	Superintendent	Orange Park
Helen S. Morford	Secretary	New Haven

PUBLICATIONS FROM LABORATORIES OF COMPARATIVE PSYCHO-BIOLOGY OF YALE UNIVERSITY (AND FROM ORIGINAL PRIMATE LABORATORY)

1925–1931[9]

Donald K. Adams.　The inference of mind.　*Psychol. Rev.*, 1928, 35: 235–252.

　　Experimental studies of adaptive behavior in cats.　*Comp. Psychol. Monog.*, 1929, vol. 6, no. 1.　Pp. 168.

Harold C. Bingham.　Parental play of chimpanzees.　*J. Mammal.*, 1927, 8: 77–89.

　　Sex development in apes.　*Comp. Psychol. Monog.*, 1928, vol. 5, no. 1.　Pp. 165.

　　Chimpanzee translocation by means of boxes.　*Comp. Psychol. Monog.*, 1929, vol. 5, no. 3.　Pp. 91.

　　Selective transportation by chimpanzees.　*Comp. Psychol. Monog.*, 1929, vol. 5, no. 4.　Pp. 45.

　　Observations on growth and development of chimpanzees.　*Amer. J. Phys. Anthrop.*, 1929, 13: 433–468.

Margaret S. Child [Mrs. Margaret Child Lewis] (with R. M. Yerkes).　Anthropoid behavior.　*Quart. Rev. Biol.*, 1927, 2: 37–57.

Joseph A. Gengerelli.　The principle of maxima and minima in animal learning.　*J. Comp. Psychol.*, 1930, 11: 193–236.

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Chauncey M. Louttit (with Raymond Dodge).　Modification of the pattern of the guinea pig's reflex response to noise.　*J. Comp. Psychol.*, 1926, 6: 267–285.

　　Reproductive behavior of the guinea pig.　I. The normal mating behavior.　*J. Comp. Psychol.*, 1927, 7: 247–263.

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　　Reproductive behavior of the guinea pig.　II. The ontogenesis of the reproductive behavior pattern.　*J. Comp. Psychol.*, 1929, 9: 293–304.

　　Reproductive behavior of the guinea pig.　III. Modification of the behavior pattern.　*J. Comp. Psychol.*, 1929, 9: 305–315.

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[9] These publications are based wholly or in part on work done in the laboratories.

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(With Carl G. Hartman.) Behavioral aspects of parturition in the monkey (*Macacus rhesus*). *J. Comp. Psychol.*, 1930, 11: 63–98.

(With R. R. Squier and Carl G. Hartman.) The fetal heart rate in the monkey (*Macacus rhesus*). *Proc. Soc. Exp. Biol. and Med.*, 1930, 28: 285–288.

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Scientific method in making laws. *State Government*, 1930, 3: 3–8.
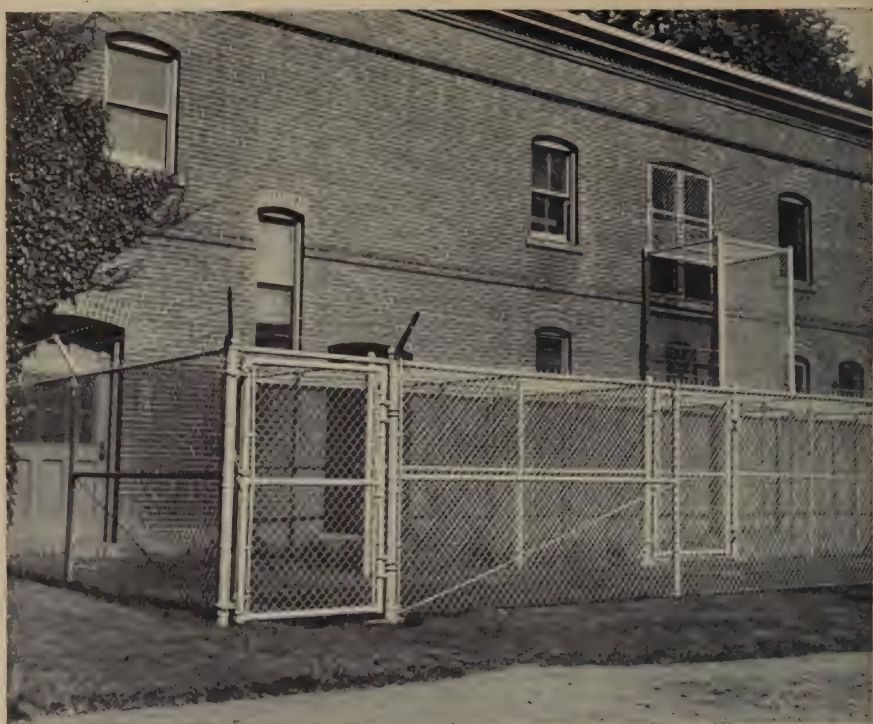
PLATE 1. Views of old Primate Laboratory, New Haven, to show open-air cages. (Photographed by R. M. Yerkes.)
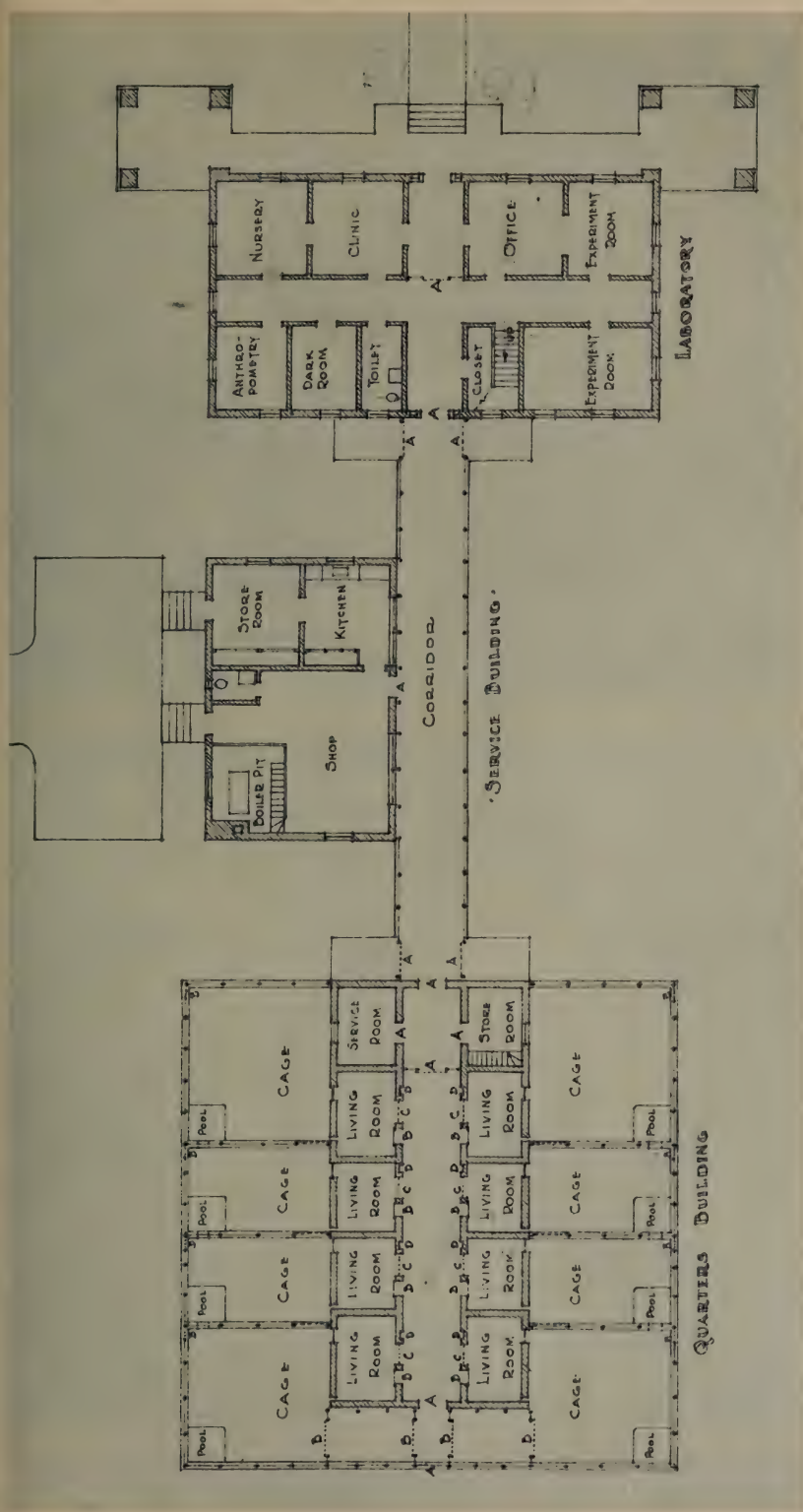
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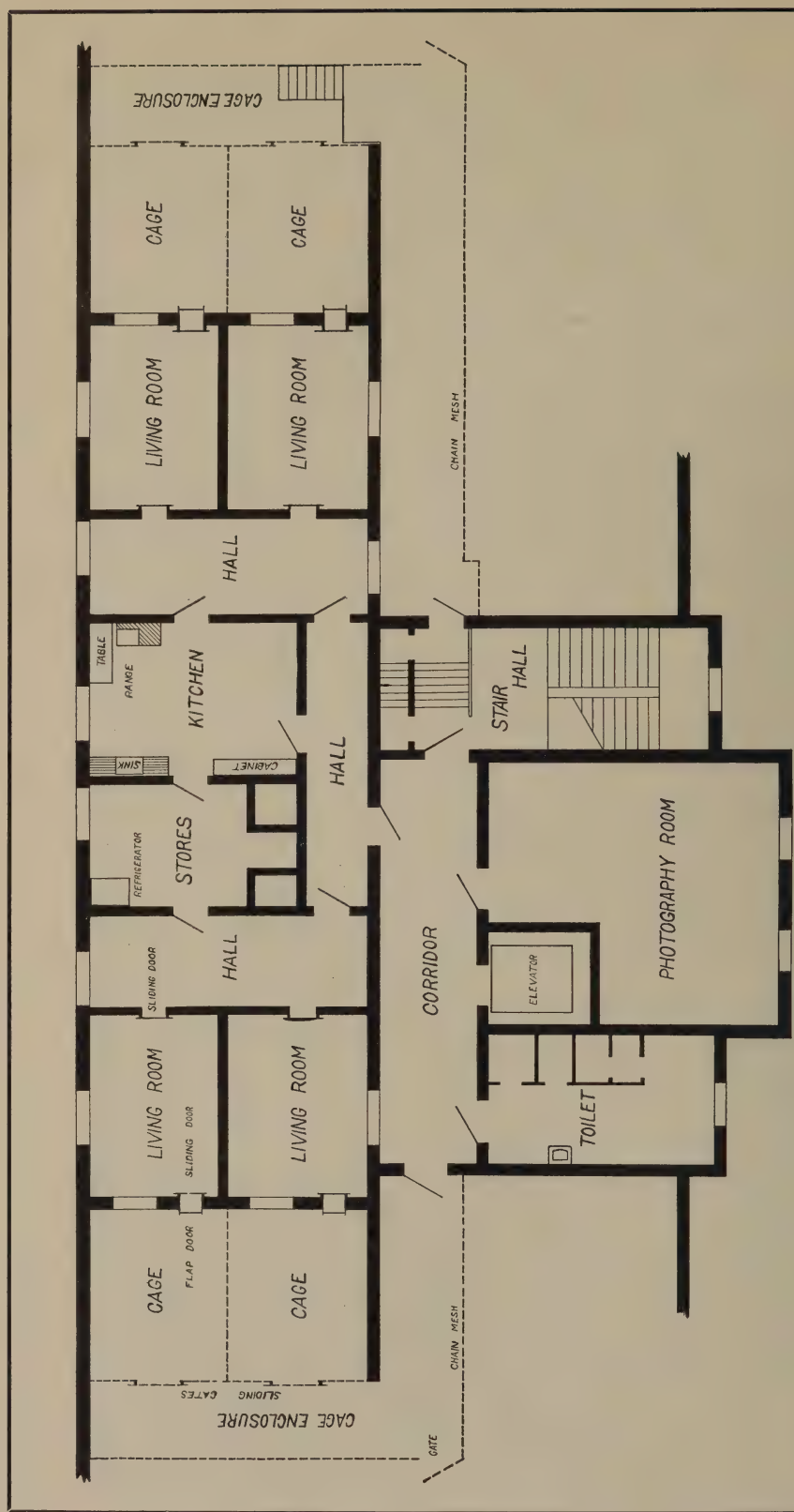
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COMPARATIVE PSYCHOLOGY MONOGRAPHS

REWARD AND PUNISHMENT IN ANIMAL LEARNING

BY

EDWARD L. THORNDIKE

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REWARD AND PUNISHMENT IN ANIMAL LEARNING[1]

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CONTENTS

§1. THE PURPOSE OF THE EXPERIMENTS

In "The Fundamentals of Learning" ('32) we presented evidence which led to the following conclusions:

"First, a satisfying after-effect which belongs to a connection can be relied on to strengthen the connection.

"Second, an annoying after-effect under the same conditions has no such uniform weakening effect. In certain cases, known by general observation or displayed in experiments such as those of Hoge and Stocking and of Warden and Aylesworth, an annoying after-effect does weaken the tendency which produces it.

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[1] The investigation reported here is a part of a general study of interests and motives made possible by a grant from the Carnegie Corporation.

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"Third, when it does so, its method of action is often, perhaps always, indirect. That is, the person or animal is led by the annoying after-effect to do something else to the situation which makes him later less likely to follow the original connection.

"Fourth, what he is led to do indirectly is often (1) either to make a native or acquired response to the particular annoyance in question (as when he responds to annoyance at a certain place by leaving that place, or to annoyance by a certain object by avoiding that object, or to annoyance in the mouth by spitting out the mouth's contents), or (2) to have an idea or other awareness of the undesirability of such and such behavior (as when he responds to a 'Wrong' heard after saying that 9×8 are 78 by thinking '78 is not good to say for 9×8').

"Fifth, what an animal is led to do indirectly by an annoyer need not make him later less likely to follow the original connection. For example, let an animal that has learned to choose exit A rather than B or C or D from a certain pen nine times out of ten because A has meant rest and food whereas B, C, and D have meant only rest, be given, the next time it enters B, a violent electric shock, producing a panic of agitation and terror. Then in later trials the animal may be so agitated and panic stricken when put in the pen that it is as likely to go to exit B as to exit A, increasing the frequency of that error from 0.10 to 0.25.

"The influence upon learning of both satisfiers and annoyers depends upon what they cause the animal to be or do. A satisfier which is attached to a modifiable connection always, or almost always, causes the animal to be or do something which strengthens the connection to which the satisfier is attached; but we do not know what this something is. It may be to maintain relatively undisturbed the physiological basis of the connection; it may be to retain it longer than would otherwise be the case; it may be to confirm it by some metabolic effect; it may be to alter it in some more mysterious way. An annoyer which is attached to a modifiable connection may cause the animal to feel fear or chagrin, jump back, run away, wince, cry, perform the same act as before but more vigorously, or whatever else is in his repertory as a response to that annoyer in that situation. But there is no evidence that it takes away strength from the physiological basis of the connection in any way comparable to the way in which a satisfying after-effect adds strength to it."

In particular, the evidence from the animal experiments of Kuo, Yerkes, Coburn and Yerkes, and Sadovinkova indicated

that the negative effect of one occurrence of a connection punished by confinement was between 0 per cent and 13 per cent of the positive effect of one occurrence of a connection rewarded by food.

These experiments were excellent for the purpose in the sense that they were absolutely free from bias in favor of our hypothesis, being designed without regard to it and by investigators who had confidence in the power of punishment to produce learning. They were not satisfactory in that the number of animals was small and that the learning was often slow, subtle, and subject to facilitation and interference from previous learning.

It is the purpose of the present monograph to report experiments which will provide data on the influence of rewards and punishments that will be a check upon our earlier determinations. They will also enable us to make progress in analyzing the effect of punishments.

The method throughout is that of using a multiple-choice apparatus in which certain responses (a) are rewarded by freedom and food, certain others by (b) confinement for 30 seconds, and certain others by (c) what may be called thwarting. The subjects of the experiments were chicks 13 to 50 days old.

§2. THE EXPERIMENTS

In all of the six experiments to be described in this section, the choice was between three acts differing chiefly in the location where the act was performed. In all six the animal was put in a small box leading to a large choice-chamber at the end of which were six alleys, or doors, or hurdles, or openings, three of which were always shut off from any possibility of response, leaving three which he might go into, or push against, or jump upon, or try to squeeze through. The six situations were differentiated for the animal by the shape and appearance of the choice-chamber and by the appearance of the end where the three alleys, doors, hurdles, or openings were. Fig. 1 shows the arrangement for each of the six. The three choices are denoted as 1, 2 and 6 or 1, 5 and 6 according to their position from left to right of an animal facing them.

In experiment AA the hurdles were pieces of wood 12 inches high and $\frac{1}{4}$ inch thick with a space cut out 6 inches by $2\frac{1}{2}$ inches. The chick thus had to jump up to an opening 6 inches by $2\frac{1}{2}$ inches, at a level 6 inches above the floor.

In Experiment AA, if the animal jumped upon hurdle 2 and down into alley 2, a wooden slide at the food-pen end of the alley was opened allowing him to escape to the food-pen where he had freedom, food and company for 60 seconds.[2]   If he jumped upon hurdle 1 and down into alley 1, a wooden slide was inserted behind him and he was left confined in the alley for 30 seconds. Similarly for hurdle 6.   If he did not himself jump down into alley 1 or 6 after jumping on the hurdle, he was knocked down from behind and left confined as before.   AA then has as reward 60 seconds freedom, food and company, and, as punishment, 30 seconds of confinement.   An animal had ten trials in immediate succession, being put into a pen with no food after the confinement of trial 10, if that was a 1 or 6.   After eleven days, he had ten more trials in immediate succession.   In the case of some animals there was a third set of ten trials after a further interval of four days.   A similar time arrangement (7 to 14 days between trial 10 and trial 11, and 2 to 5 days between trial 20 and trial 21) holds good for experiments BB, CC, DD, EE, and FF, also, save for a very few irregularities due to some special circumstances.

In Experiment BB the three acts were simply going into alley 1, 5, or 6 to a distance of 2 or 3 inches.   In the case of 1, a wooden slide at the food-pen end was lifted, allowing the animal to go on to the food-pen and have 60 seconds of freedom, food, and company.   In the case of 5 and 6, a wooden slide was inserted behind the animal, leaving him confined in the alley for 30 seconds. The reward and punishment were thus the same in BB as in AA.

Experiment FF was like BB, except that the act consisted not only of entering alley 1, 2, or 6 but also of going along the alley 10 or 12 inches, and up an inclined plane, or block, or flight of steps in the alley.   The three alleys differed markedly in appearance, the plane in 1 being long and ending in a platform and

[2] Two or more chicks, usually four or five, were always present in the food-pen to provide the feature of company.
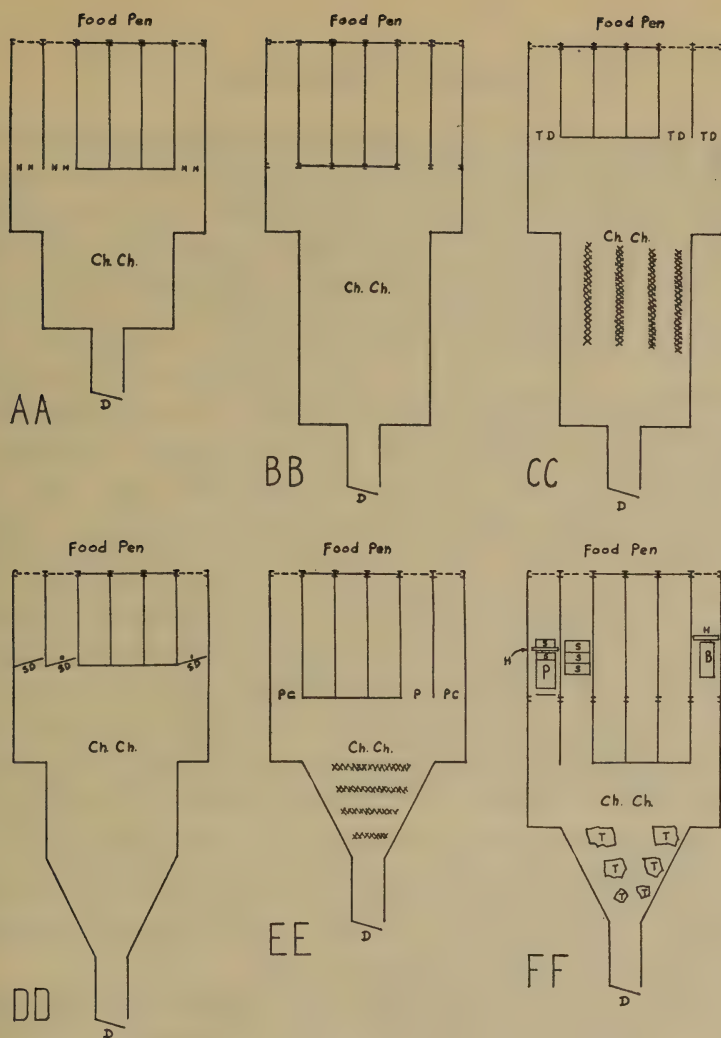
FIG. 1. AA. Apparatus AA. HH = hurdle. Ch.Ch. = Choice chamber. D = door at which chick was put in.

FIG. 1. BB. Apparatus BB. Ch.Ch. and D. as in AA.

FIG. 1. CC. Apparatus CC. TD = door hinged at the top. Ch.Ch. and D as in AA. On the floor of the choice chamber there was laid a board with four wide black stripes, shown here as cross-hatchings.

FIG. 1. DD. Apparatus DD. SD = door hinged at the side. Ch.Ch. and D as in AA.

FIG. 1. EE. Apparatus EE. P = tissue paper. PC = tissue paper backed by card-board. Ch.Ch. and D as in AA. On the floor of the choice chamber there was laid a board with black stripes, shown here by cross-hatching.

FIG. 1. FF. Apparatus FF. H = hurdle. P = inclined plane. S = step. B = block. T = red-tile to aid identification of the apparatus. Ch.Ch. and D as in AA.

The scale for all is 1 to 12.

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hurdle (4 inches tall) with a platform on the farther side also, the steps in 2 being short and low ($2\frac{1}{4}$ inches tall in all), the block in 6 being narrow and ending in a 4 inch hurdle.   Six was rewarded; 1 and 2 were punished by confinement.   Each of AA, BB, and FF is thus a simplified case of choice among three acts alike in general nature but different in the location where the act is performed and (in FF)in its particular nature.   The reward and punishment are identical for AA, BB, and FF.   The punishment is confinement (to which, it may be noted, the chicks reacted by typical signs of discomfort, such as loud cheeps and jumping).

In Experiment CC, the choice was between three doors each $4\frac{3}{4}$ inches high and $2\frac{5}{8}$ inches wide hung from the top in a space $5\frac{3}{4}$ inches by $3\frac{1}{2}$ inches.   One (6) would swing back letting the animal into the alley if he pushed against it steadily.   The other two (1 and 2) were held by pegs so that they could be pushed back not at all at the center and only about an eighth of an inch at either side.   If, then, the animal pushed against 6 steadily or tried to squeeze under 6 vigorously, he would enter alley 6, in which case a glass slide at the food-pen end of alley 6 was opened allowing him to escape to the food-pen, where he had freedom, food, and company for 60 seconds.   If he pushed against 1 or 2, he was punished only to the extent that any tendency to escape from the choice-chamber which may have led to the act was thwarted.[3]   His confinement in the choice-chamber continued, but nothing else punitive happened to him.

In Experiment DD, the choice was between three doors each $3\frac{3}{4}$ inches high by $2\frac{1}{2}$ inches wide set in an opening 5 by 3, hung by hinges at the left side.   All were set partway open so as to leave a space of about $\frac{3}{4}$ inches between the edge of the door and the edge of the alley.   One (1) would swing further back letting the animal into the alley if he pushed against it or tried to squeeze through the opening ($\frac{3}{4}$ inch by 5 inches) at its right side with sufficient vigor.   The other two doors (2 and 6) were prevented

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[3] Too feeble pushing or squeezing at 6 was, in general, punished similarly.   A squeezing under 6 which was not continued long enough was followed by similar punishment plus also occasionally the discomfort of having one's head caught for a second or so.

from moving further back by pegs. If the animal pushed door 1 open and went into alley 1, a glass slide at the food-pen end was lifted, and the usual reward of 60 seconds freedom, food and company ensued. If he pushed at 2 or 6, he was punished, as in CC, only to the extent that any tendency to escape from the choice-chamber which may have led him to do so was thwarted.[4] His confinement in the choice-chamber continued, but nothing else punitive happened to him.

In Experiment EE, the choice was between trying to squeeze through a rectangular opening 4 inches tall by $\frac{3}{4}$ inch wide cut in a white expanse 7 inches tall and 4 inches wide which covered the entrance to alley 1, or 2, or 6, respectively. This cut was made in soft tissue paper in the case of 5 and in soft tissue paper backed up by stiff cardboard in the case of 1 and 6. In the case of 5 the paper would easily give or tear permitting entrance to alley 5. In the case of 1 an 6, the cardboard prevented entrance. In the case of squeezing through 5, a glass slide at the food-pen end of alley 5 was lifted, allowing the chick exit to the usual reward. In the case of attempts to squeeze through 1 or 6, the punishment was only whatever the failure to get out of the choice-chamber involved.

Each of CC, DD, and EE is thus a case of choice among three acts alike in nature, but differing in the location where the act is performed. The reward and the punishment are identical for CC, DD, and EE. The reward is the same as for AA, BB, and FF. The punishment is very different. In CC, DD, and EE it consists in a continuance of the confinement in the choice-chamber, with the possibility at any second of escape by attack on the "right" door or opening, and in the thwarting of whatever tendencies to escape from the choice-chamber led to the act. In AA, BB, and FF it consisted in a much narrower confinement, with no possible escape, for 30 seconds, followed by readmission to the confinement of the choice-chamber.

The punishments of CC, DD and EE differ from those of AA, BB, and FF also in the behavior to which they lead. In CC, DD,

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[4] Too feeble pushing or squeezing at 1 was punished similarly.

and EE, the failure of the pushing or squeezing at the "wrong" door or opening may lead to withdrawal from it, and, indirectly, to attack upon one of the other two doors or openings. It may lead to looking at it without pushing it or trying to squeeze through it. But in AA, BB, and FF the confinement in the alley prevents the animal for 30 seconds from forming *directly* any connection whatever with anything in the choice-chamber. He cannot withdraw from the choice-chamber side of the hurdle or alley entrance. He cannot even see any of these. The actual situation has vanished.

In AA, BB, and FF the punishment and the reward are comparable in that by the time they arrive, the situation and response, the connection between which is punished or rewarded, are inaccessible in reality. In CC, DD, and EE this is true only of the situation and response whose connection is rewarded. In the case of punishment, the external situation persists, and the animal may be led to back away from the door, or turn aside from it, or neglect it, etc.

This is true when the technique is carried out perfectly. Unfortunately, at times a chick will jump back or run back out of an alley in AA, BB or FF before the slide can be inserted behind him. Consequently the learning is to a slight extent influenced by retreats from the "wrong" alleys in AA, BB, and FF.

Besides the three acts with which we are primarily concerned an animal may respond in any of the six choice-chambers by running around the chamber, cheeping, jumping at the confining walls, pecking at various objects, voiding urine or excrement, squatting down and other acts within its repertory. Its behavior was observed and in some cases various instructive features of it were recorded. But the records to be used here for any chick consist simply of the time it was put into the entrance-box, the fact and time of occurrence of the specific "right" and "wrong" acts during the day's experiment until ten had occurred, or until two (in some series, 1 or 3) periods of failure to do any one of them in 300 seconds had been spent in the choice-chamber or until eight had occurred plus two such periods of failure, or until nine had occurred plus one such period of failure.

§3. THE RECORDS

We have then for each chick in each experiment the record shown in Table I.[5] Except for rare cases of illness of a chick or some inadvertence, trials 1 to 10 in any experiment were, as noted heretofore, all on the same day and in immediate succession. Then came an interval of 7 days or more in which the other five experiments were done. Then came trials 11 to 20 all on one day and consecutive. Then after 4 days or more came, for some chicks in some experiments as shown in Table I, trials 21 to 30. The lengths of the intervals were as follows:

Between trial 10 and trial 11:

> Experiment AA, chicks 1–47, 11 days; chicks 48–61, 11 days; chicks 56–107, 7 days.
> Experiment BB, chicks 1–47, 10 days; chicks 48–61, 12 days; chicks 56–107, 7 days.
> Experiment CC, chicks 1–47, 11 days; chicks 48–61, 12 days; chicks 56–107, 6 days.
> Experiment DD, chicks 1–47, 9 days; chicks 48–61, 13 days; chicks 56–107, 7 days.
> Experiment EE, chicks 1–47, 11 days; chicks 48–61, 12 days; chicks 56–107, 11 days.
> Experiment FF, chicks 1–47, 12 days; chicks 48–61, 14 days; chicks 56–107, 12 days.

Between trial 20 and trial 21:

> Experiment AA, chicks 48–61, 4 days.
> Experiment BB, chicks 48–61, 3 days, chicks 65–107, 4 days.
> Experiment CC, chicks 48–61, 4 days, chicks 65–107, 5 days.
> Experiment DD, chicks 48–61, 3 days, chicks 65–107, 4 days.
> Experiment EE, chicks 48–61, 3 days, chicks 65–107, 5 days.
> Experiment FF, chicks 48–61, 2 days, chicks 65–107, 5 days.

Table I gives the records of the chicks whose records were used in this report, including all who lived long enough to do experiments AA to FF. The others died, many before the experiments began, and some during their course (including twenty killed by a dog).

Chicks 1 to 47 began Experiment AA when 18 days old, had

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[5] We have also a record of the times in all cases. But these will not be referred to in the present report.

TABLE I

The response of each chick in each trial of Experiments AA, BB, CC, DD, EE and FF

Experiment AA.   Response 2 was rewarded

CHICK	SERIES A, NORMALLY TRIALS 1 TO 10										SERIES B, NORMALLY TRIALS 11 TO 20										SERIES C, NORMALLY TRIALS 21 TO 30									
1	6	2	6	6	2	2	2	2	2	6	6	6	6	6	6	2	2	2	2	2										
6	6	6	6	6	6	6	6	6	2	2	6	1	2	2	2	2	6	2	2	2										
7	6	1	1	1	1	2	1	2	2	2	2	2	6	2	6	2	6	2	2											
10	6	6	6	6	2	2	6	2	2	6	2	6	2	2	6	2	6	6	6	6										
11	2	2	2	2	1	6	1	6	6	2	2	2	2	2	2	2	2	2	2	2										
12	6	2	2	2	2	2	6	6	6	6	6	6	2	2	2	6	6	6	2	2										
14	1	2	1	2	2	2	1	2	2	2	6	6	6	6	6	6	6	2	2	6										
18	1	6	1	1	1	1	2	1	6	2	6	6	2	F	F	1	1	2	1	6										
19	6	6	6	F	F	6	F	F			6	6	6	6	6	6	2	6	6	2										
20	2	2	6	2	2	2	6	2	2	2	2	2	2	2	2	2	2	2	2	2										
21	6	6	2	2	1	6	6	2	2	2	6	2	2	2	2	6	2	6	6	1										
23	F	F	F																											
24	2	6	2	2	2	2	2	2	6	2	6	2	6	6	2	6	6	6	6	2										
26	2	2	2	2	2	2	2	2	2	2	2	2	1	1	2	2	2	6	6	2										
27	2	6	2	2	2	2	2	6	2	2	1	2	1	2	F	2	2	2	2	2										
29	6	2	1	6	2	1	6	6	2	6	6	6	6	2	2	2	2	2	2	2										
31	F	F									2	6	2	6	6	6	6	6	1	6										
34	1	6	2	2	2	6	2	2	2	2	6	2	2	2	2	6	2	2	2	2										
37	2	6	6	2	2	6	6	1	6	6	1	6	6	2	2	6	6	2	1	2										
41	2	6	2	2	6	2	2	2	2	2	2	2	6	6	2	2	2	6	2	2										
47	6	2	2	2	2	2	2	2	2	2	6	6	6	1	6	1	6	F	F	6										
48	2	2	2	2	2	6	6	6	2	6	6	6	6	6	1	6	F	6	F	2	6	6	6	2	2	6	1	2	1	6
49	F	F	F	F	F	6	2	6	1	F	2	2	2	2	2	2	2	2	2	2	6	6	6	2	2	6	2	2	2	2
50	2	2	2	2	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	6	2	1	2	1	2	2
51	6	6	2	2	F	1	F	F			F	F									F	F								
52	6	2	6	2	6	2	2	2	2	2	6	6	2	6	2	6	6	6	2	6	6	2	2	2	2	6	6	1	6	6
53	2	2	2	6	6	2	2	2	2	2	1	2	1	2	1	1	F	1	1	6	6	2	2	6	1	1	2	6	6	6
55	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
56	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6	2	2	2	2	2	2	6
57	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6	6	6	2	6	2	2
58	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	1	2	2	2	2	2	2	2	2	2	2	2	2	2
59	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
60	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6	2	2	2	2	2	2	6	2	2
61	2	2	2	2	2	6	2	2	2	2	6	1	2	2	2	2	2	2	2	2	6	2	2	2	2	6	2	2	2	2
65	6	2	6	1	2	2	2	2	2	2	2	6	2	2	2	2	2	2	2	2										
66	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	6	6	2										
67	F	2	2	6	F						2	6	2	6	2	2	6	2	2	6										
68	6	F	F								F	1	1	F																
69	6	6	6	6	6	6	6	6	F	2	6	1	2	2	2	2	2	2	2	2										
70	2	2	2	2	2	2	2	2	2	2	6	2	2	2	2	2	2	2	2	2										
71	F	F									F	F																		
73	2	1	2	1	1	2	2	1	2	1	2	2	2	2	2	2	2	2	2	2										
75	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
76	F	F									F	F																		
77	1	2	2	6	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
78	F	2	2	6	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
79	2	2	2	2	2	2	2	2	2	2	6	2	2	2	2	2	2	2	2	2										
80	6	1	2	2	2	2	2	2	2	2	2	6	2	2	2	2	2	2	2	2										
82	F	6	6	F							F	6	1	6	6	1	F													
83	2	2	2	2	2	2	2	1	2	2	2	1	2	2	2	2	2	2	2	2										
85	2	F	F								F	F																		
86	F	F									2	2	2	2	2	2	1	6	1	1										
87	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
88	2	2	2	2	6	2	2	6	2	1	2	2	2	2	2	2	2	2	2	2										
93	2	2	2	2	2	6	1	2	2	2	2	2	2	2	1	2	6	2	2	2										
95	6	6	6	6	6	6	6	6	6	6	6	6	2	2	2	6	2	2	2	2										
96	2	2	2	2	2	2	2	2	2	2	6	6	2	2	2	2	6	6	6	2										
98	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
100	2	6	2	1	F	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										
101	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	2	2										
103	2	2	2	2	2	2	2	2	2	2	6	2	2	6	2	2	2	2	2	6										
104	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	1	2										
105	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6										
107	F	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2										

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TABLE I—*Continued*

Experiment BB. Response 1 was rewarded

CHICK	TRIALS 1 TO 10										TRIALS 11 TO 20										TRIALS 21 TO 30									
1	5	5	5	6	5	5	5	5	6	5	5	6	5	5	5	1	1	6	5	1										
6	5	5	6	5	5	5	5	5	5	5	5	1	5	5	5	1	1	1	5	5										
7	5	F	1	5	6	6	6	1	1	5	5	5	6	5	1	5	1	5	1	5										
10	6	5	5	5	1	5	5	1	5	5	5	5	1	5	1	1	1	1	1	1										
11	6	5	5	F	5	1	6	6	6	6	1	1	1	1	1	1	1	1	1	1										
12	5	5	6	5	1	6	5	5	F	6	5	5	6	6	5	5	1	1	5	1										
14	5	5	5	5	5	1	5	5	5	1	5	5	5	5	1	1	1	1	5	5										
18	1	1	1	5	1	1	1	1	1	1	5	5	1	1	1	1	1	1	5	5										
19	5	5	5	5	5	6	5	1	5	1	5	5	5	5	1	5	5	1	1	1										
20	5	5	6	6	6	5	1	5	5	1	1	5	5	5	5	5	1	5	5	5										
21	1	1	5	5	5	5	1	1	1	5	1	5	1	5	5	5	5	5	5	1										
23	5	1	5	5	1	5	1	5	1	5	5	1	5	1	5	1	1	5	1	1										
24	5	5	5	5	6	6	6	1	5	1	1	1	5	5	5	6	6	5	5	1										
26	1	1	5	5	5	1	1	5	5	1	5	1	6	5	5	1	5	1	1	1										
27	5	6	5	6	F	6	5	1	5	1	5	5	1	5	1	1	1	1	1	1										
29	1	5	5	5	5	5	5	6	1	1	5	5	5	1	5	1	1	1	1	1										
31	1	1	1	5	5	1	1	1	5	1	5	6	1	5	5	5	1	1	1	1										
34	1	1	1	5	5	5	5	5	1	1	5	5	1	5	5	5	5	1	1	1										
37	5	5	1	5	5	5	1	5	5	5	5	6	5	5	1	5	5	5	1	1										
41	5	5	1	1	6	5	5	5	5	5	1	5	1	1	1	1	1	1	1	1										
47	5	6	5	5	5	5	5	5	5	1	5	1	1	1	5	5	5	5	5	1										
48	5	5	5	1	5	5	5	F	5	6	5	1	1	5	1	1	1	5	1	1	6	6	6	1	1	1	1	1	1	1
49	1	5	5	5	1	5	5	6	5	5	5	1	1	1	1	5	1	1	1	1	6	5	1	1	1	1	1	1	1	1
50	5	5	5	5	1	5	5	6	6	1	5	5	1	1	1	1	1	1	1	1	1	1	1	5	1	1	1	1	1	1
51	1	5	1	5	1	F	F				F	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
52	1	1	5	1	1	1	1	1	1	1	1	1	1	1	1	1	5	1	1	1	1	1	1	1	1	5	5	5	5	1
53	5	5	5	5	5	5	5	6	6	5	F	5	F								5	6	6	6	6	5	5	5	6	6
55	1	5	5	F	5	5	6	1	1	1	5	5	5	5	5	5	5	6	5	5	1	1	1	5	6	1	5	1	6	1
56	1	1	1	1	5	5	5	1	5	5	5	5	5	5	5	1	5	5	5	5	1	5	5	5	1	5	5	6	6	5
57	1	5	1	1	5	5	1	5	5	1	1	1	5	5	1	1	1	5	5	1	1	1	5	6	1	1	1	1	1	1
58	1	1	1	5	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	5	1	5	5	1	1	5	1	1
59	5	1	1	1	5	5	5	5	1	1	5	6	1	1	1	1	1	1	1	1	5	6	1	1	1	1	1	1	1	1
60	5	5	1	1	1	5	1	6	5	5	1	5	1	6	1	1	5	F	5	1	1	5	5	1	6	5	1	5	1	5
61	5	5	5	1	5	5	6	5	1	5	1	5	6	5	5	5	1	1	1	1	1	1	5	5	6	6	6	1	5	1
65	5	5	5	6	6	5	5	5	6	1	6	6	5	1	1	5	6	1	1	6	1	1	1	1	6	1	5	1	1	1
66	5	5	5	6	1	1	5	5	1	1	5	1	6	1	1	1	1	1	1	5	1	1	1	5	1	1	1	1	1	1
67	6	5	5	F	1	5	5	1	1	5	5	1	6	5	1	1	1	6	1	1	1	5	5	5	5	1	1	1	1	1
68	5	5	6	6	5	6	6	F	1	5	5	6	6	6	6	1	5	1	1	5	1	5	1	1	1	1	1	1	1	1
69	5	6	5	6	5	5	1	5	6	1	5	5	6	6	5	1	5	1	1	5	5	1	6	1	1	5	6	1	1	5
70	5	5	5	5	6	6	1	1	6	1	1	1	6	1	1	1	1	5	6	1	1	1	5	1	1	5	1	1	1	1
71	5	5	5	5	5	6	1	1	1	5	1	1	5	6	F	1	1	1	1	5	1	5	1	1	1	5	1	1	1	1
73	1	1	1	1	1	1	1	1	5	5	6	1	1	6	1	1	1	1	1	1	1	1	5	1	1	1	1	1	1	1
75	5	5	5	5	5	5	5	5	5	5	6	5	6	6	6	6	1	1	5	1	1	5	5	5	1	5	5	5	5	5
76	5	5	1	5	F	5	5	5	1	5	5	5	5	5	5	1	5	5	5	1	1	5	5	1	1	1	1	1	1	1
77	5	1	1	5	5	1	1	1	1	1	1	5	5	6	6	1	1	5	1	1	1	5	5	1	1	1	1	1	1	1
78	5	5	5	5	5	6	F	5	1	5	5	5	5	5	6	5	5	1	5	5	6	6	5	5	6	1	1	1	1	1
79	1	1	1	5	1	1	1	1	1	1	5	5	1	5	5	6	6	1	1	5	6	5	5	5	6	5	1	1	5	1
80	5	5	5	5	1	1	5	6	1	1	6	6	1	6	1	1	1	1	1	1	1	5	1	5	1	5	1	5	1	5
82	5	1	5	5	5	6	1	6	1	6	1	6	1	1	1	1	6	1	1	1	1	1	1	1	1	1	1	1	1	1
83	5	5	1	1	1	1	1	1	1	1	5	6	6	6	5	5	1	1	1	1	1	1	1	1	1	1	1	1	1	1
85	6	5	5	5	6	6	1	5	5	1	6	6	1	1	1	1	1	1	1	5	5	6	6	6	1	6	1	6	1	1
86	5	5	F	5	5	5	5	F	—	—	5	5	6	6	5	5	6	1	6	5	5	1	1	1	5	5	5	1	5	1
87	5	1	1	6	1	1	1	1	1	1	6	1	1	1	1	5	1	1	1	1	1	1	1	1	1	1	1	1	1	1
88	5	1	5	5	5	5	5	6	1	1	1	1	5	5	1	5	5	1	6	1	5	1	1	5	1	1	6	5	1	5
93	5	6	1	5	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	5	1	1	1	5	6	1	1
95	5	5	6	5	5	5	6	5	5	1	5	6	5	6	1	1	1	6	5	5	6	1	5	1	1	5	6	1	1	1
96	5	5	5	6	5	5	5	5	1	6	6	6	6	1	1	1	1	1	1	1	5	6	1	1	1	5	1	1	1	5
98	1	5	1	5	5	5	5	6	1	5	5	5	1	6	1	1	6	1	1	1	5	6	6	5	1	5	1	1	1	5
100	1	5	5	F	1	1	5	1	1	1	1	1	5	6	1	1	5	1	1	1	1	1	1	1	1	5	1	1	1	1
101	5	5	5	5	5	5	5	F	1	1	5	F	5	5	5	5	6	6	5	5	1	6	F	1	1	5	1	1	6	1
103	5	6	6	5	5	F	F	—	—	—	5	6	5	6	5	5	5	6	5	5	1	5	1	1	1	1	1	5	1	
104	5	1	5	5	1	5	5	F	1	1	1	6	1	6	1	1	1	1	5	6	5	6	6	6	6	6	6	1	1	1
105	1	1	1	1	1	1	1	1	1	1	1	1	5	6	1	1	1	5	6	1	1	1	1	1	1	1	1	5	1	1
107	5	5	6	1	5	1	1	1	1	1	5	5	5	6	1	6	5	6	6	1	1	5	1	1	1	5	1	1	5	

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TABLE I—*Continued*

Experiment CC.   Response 6 was rewarded

Values are listed left-to-right for each block of trials.

CHICK	TRIALS 1 TO 10	TRIALS 11 TO 20	TRIALS 21 TO 30
1	5 F F 6 5 5 6 6 6 6	6 5 6 6 6 6 6 6 6	
6	1 1 1 1 5 1 1 1 5 5	F F F 6 1 5	
7	6 6 6 6 6 6 5 6 6 6	6 6 6 6 6 6 6	
10	F F F	F F 1 1 5 F	
11	5 6 6 5 5 5 6 6 6 5	5 5 6 5 1 5 6 6 1 6	
12	F F F	F F F	
14	F F F	5 5 F 5 1 1 5 5 1 5	
18	1 F F F	5 6 6 5 6 5 6 6 5 6	
19	5 5 5 6 5 1 5 5 5 6	1 1 5 5 5 1 5 6 6 5	
20	5 5 5 5 5 5 6 5 6 6	5 5 5 5 5 5 5 5 5 5	
21	F F 6 6 6 6 6 6 6 6	6 6 6 6 6 6 6 6 6 6	
23	F	F F F	
24	6 1 1 6 6 6 6 6 6 6	1 1 6 6 6 6 6 6 6 6	
26	F F F	F 6 1 1 5 5 5 1 1 1	
27	5 5 5 F F F F	F F F	
29	5 5 5 F F 5 5 6 6 5	6 1 1 6 6 1 6 6 6 6	
31	1 1 1 6 1 1 6 6 6 6	1 6 6 6 6 1 6 6 6 6	
34	5 F F F	F F F	
37	5 1 1 1 1 1 6 5 6 6	5 6 6 6 6 6 6	
41	1 1 F 5 1 5 1 5 5 5	F F F	
47	F F F	5 5 5 F F F	
48	5 5 5 5 1 5 6 1 5 5	5 5 5 5 5 6 5 6 6 6	1 6 6 6 6 6 6 6 5 6
49	5 1 5 1 1 5 1 5 5 6	5 5 5 5 5 6 5 5 5 5	6 6 5 5 5 5 5 5 F 6
50	5 1 1 1 1 5 5 5 6 5	5 5 5 6 5 6 6 5 6 6	5 6 1 1 6 1 6 5 5 6
51	5 5 5 6 5 5 5 6 5 5	5 1 1 5 6 6 6 6	6 6 F 5 6 6 6 6 6 6
52	5 5 6 5 5 5 5 5 5 5	5 F F	5 5 5 5 F F
53	F F 5 5 F	F F	1 F F F
55	5 5 5 6 5 5 5 5 5 5	5 5 5 5 F 5 F	F F
56	5 1 5 1 6 5 F F	F F	F F
57	1 1 5 F 1 F F	5 5 5 5 F 5 F	1 5 1 1 1 F 5 1 5 1
58	6 1 1 5 5 5 F F	F F	F F
59	1 1 1 1 1 1 1 1 6 1	5 5 5 1 6 6 1 6 6 6	5 6 6 6 6 6 6 6 6 6
60	1 6 1 1 1 1 1 5 5 6	5 5 1 1 1 6 5 5 6 1	5 5 5 1 5 5 5 5 5 5
61	1 1 1 1 1 5 5 6 6 5	5 6 5 6 6 5 6 6 6 6	6 6 6 6 6 6 6 6 6 6
65	1 5 6 5 6 6 5 5 6 1	5 5 1 1 1 1 1 6 5 1	6 6 5 1 6 6 5 6 6 6
66	5 1 5 1 5 F 1 5 1 F	1 1 1 1 5 F	F F
67	F F	F	F F
68	5 5 5 F F	5 1 1 1 5 1 1 1 1 5	1 1 1 F 1 1 1 1 5
69	5 5 F 6 5 1 6 6 6 5	6 5 5 5 5 6 5 1 6 6	5 6 6 6 6 6 6 6 6 5
70	5 1 5 1 5 5 1 5 1 5	5 5 6 6 5 1 5 5 6 1	5 5 5 1 5 5 1 6 5 1
71	1 5 6 5 6 5 5 5 1 5		5 5 F F
73	5 5 5 5 5 5 5 5 5 5	5 5 5 5 5 5 5 5 5 5	
75	5 5 5 5 5 6 5 5 5 5	1 5 6 5 6 6	6 5 6 6 6 6 6 1 5 6
76	F F	1 5 F	F F
77	1 5 1 5 1 5 1 1 1 1	F	F F
78	5 5 5 F F	5 6 6 6 6 6 6 6 6 6	6 6 6 6 6 6 6 6 6 6
79	6 5 1 5 5 5 1 5 5 5	5 5 5 5 5 5 1 5 5	F F
80	F F	5 6 5 6 5 5 1 5 6 6	6 6 5 6 5 6 6 6 6 6
82	5 6 5 5 5 5 6 5 6 5	5 6 5 6 6 5 6 6 6 6	1 1 1 1 5 5 1 1 1 1
83	1 5 5 5 5 F F	5 5 5 1 5 F	F F
85	5 5 5 1 1 5 5 5 5 5	5 5 5 1 5 5 5 1 5 5	5 F F
86	5 5 5 1 5 1 5 5 5 5	5 1 5 5 1 5 5 5 5 5	F F
87	1 1 5 6 5 5 1 1 1 1	1 1 1 5 5 5 1 5 5 5	5 5 6 5 6 5 6 5 6
88	5 6 5 5 5 5 1 5 5 5	5 5 1 1 5 5 1 5 5 5	5 1 1 1 1 1 5 5 5 5
93	5 5 5 6 5 5 5 5 5 1	5 5 6 6 6 5 5 5 5 6	5 1 5 5 5 1 5 1 5 5
95	5 1 5 1 5 5 5 1 5 1	5 5 F	1 1 6 6 6 5 1 6 6 5
96	5 5 6 5 5 6 1 5 5 6	5 5 5 5 6 5 6 6 6 6	1 6 6 6 6 6 6 6 6 6
98	F F	5 5 1 1 1 5 5 5 5 5	5 5 1 5 1 1 1 F 1 F
100	1 5 1 5 5 5 5 5 F F	5 1 1 1 F	1 F F
101	5 6 5 5 5 5 5 5 5 5	5 6 5 6 5 5 6 5 5 6	1 6 5 6 1 5 6 6 6 6
103	6 6 5 6 6 5 6 6 6 6	5 5 6 5 6 5 6 5 1 5	5 1 5 5 6 1 5 6 6 6
104	5 5 5 5 F F	F	F 1 1 F
105	1 5 5 5 6 5 5 6 6 5	6 6 6 6 1 1 1 6 6 5	5 6 1 1 1 1 6 6 6 6
107	5 5 5 5 5 5 5 5 5 5	5 5 5 5 5 5 5 5 5 5	5 1 1 5 5 1 5 5 5 5

TABLE I—*Continued*
Experiment DD.   Response 1 was rewarded

CHICK	TRIALS 1 TO 10										TRIALS 11 TO 20										TRIALS 21 TO 30									
1	2	2	2	2	2	2	F	F	2	1	2	6	2	2	2	F	F	2	6	1										
6	6	6	2	2	2	6	2	1	2	2	6	1	2	2	2	2	2	2	2	2										
7	2	2	6	6	6	6	6	6	6	6	2	2	2	2	2	2	2	2	2	2										
10	2	6	2	6	6	6	6	6	6	2	F	F	F																	
11	2	2	2	2	2	2	2	1	1	2	1	1	2	1	1	2	1	2	1	1										
12	2	6	6	6	6	6	2	F	F	6	F	F	6	2	6	6	6	6	6	6										
14	2	2	2	2	6	6	6	6	2	6	2	2	1	2	1	1	1	1	1	1										
18	F	2	6	6	6	2	2	2	6	2	1	2	2	2	F	F	2	F												
19	6	2	6	6	1	2	2	2	1	1	2	2	2	2	2	2	2	6	2	6										
20	2	6	2	2	2	2	2	2	2	2	2	2	2	2	F	F	F													
21	2	2	2	2	2	2	2	2	2	2	6	6	6	F	2	6	6	6	6	6										
23	2	2	F	2	2	6	2	6	2	2	2	F	F	F																
24	2	6	2	6	2	1	6	2	2	1	2	2	2	1	2	6	2	2	2	2										
26	F	1	1	6	1	6	1	6	1	2	2	6	2	1	2	6	1	1	1	2										
27	2	2	2	2	6	6	6	6	6	2	F	1	2	1	1	2	1	2	1	2										
29	2	6	2	6	1	2	6	6	1	1	2	1	2	2	1	2	6	1	2	6										
31	2	2	2	1	1	2	2	2	6	2	2	6	1	6	2	2	6	2	6	1										
34	6	6	2	6	6	6	F	2	F	6	F	F	F																	
37	2	2	2	6	2	6	2	6	2	2	2	2	2	6	2	1	2	2	2											
41	2	2	2	F	F	F					2	1	2	2	2	6	6	6	6	F										
47	2	1	2	2	1	1	2	2	1	2	1	1	1	1	2	1	1	2	2	2										
48	2	1	2	1	2	2	2	1	2	6	2	1	2	1	2	1	2	2	2	1	1	2	1	1	2	1	2	1	1	1
49	1	1	2	1	1	2	2	2	2	2	1	2	2	2	2	1	1	2	1	1	1	2	1	1	1	1	1	1	1	1
50	1	1	1	1	1	2	2	2	2	1	2	1	2	2	2	1	2	2	1	1	1	1	1	1	2	1	2	1	2	2
51	2	2	1	1	1	2	2	2	2	2	2	2	6	2	1	2	2	2	2	1	1	1	1	1	2	1	2	1	2	2
52	2	2	1	1	1	2	6	2	6	6	2	1	2	2	1	1	1	1	1	1	1	1	1	1	1	2	1	1	1	1
53	2	1	2	2	1	2	1	1	2	2	2	1	2	1	1	1	2	2	1	1	1	1	1	2	2	1	2	2	1	1
55	2	1	2	2	1	2	1	2	2	1	2	2	2	2	1	2	2	6	6	2	2	6	2	2	1	1	1	1	1	1
56	2	1	2	1	2	1	2	1	2	1	6	2	1	2	1	2	6	2	2	1	1	6	1	6	6	6	1	1	1	1
57	1	1	2	2	2	1	2	2	1	2	1	1	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	2	2	1
58	1	1	1	2	2	1	1	6	1	1	2	2	2	2	1	2	2	1	2	2	1	2	1	2	1	2	2	2	2	2
59	2	2	2	1	2	2	2	2	2	1	2	1	2	2	1	2	1	2	1	1	2	1	1	2	1	1	2	1	1	F
60	2	2	6	6	2	2	2	6	2	2	6	6	2	6	6	2	2	6	2	2	2	6	2	6	2	6	6	6	6	6
61	2	2	2	2	6	2	2	2	2	2	2	2	6	6	2	6	6	6	6	2	6	6	2	6	2	6	6	2	6	6
65	2	2	1	1	6	1	6	2	1	6	1	1	1	1	2	1	2	1	2	2	2	6	2	2	1	2	2	2	1	2
66	1	2	1	2	2	1	1	2	1	1	2	1	2	1	2	1	2	1	1	1	1	2	2	2	2	1	1	2	1	2
67	F	F									2	2	2	2	2	2	F	F			F	F							2	2
68	6	2	2	6	2	2	2	2	6	2	6	6	2	1	2	2	6	1	2	6	6	2	2	6	1	2	2	6	2	6
69	2	2	2	2	2	2	2	2	2	2	1	6	2	2	6	1	2	2	2	2	6	2	6	2	2	2	6	6	6	6
70	2	2	2	6	1	2	6	2	6	2	2	2	2	1	2	1	2	1	2	2	2	2	2	1	2	2	2	2	1	1
71	2	2	2	2	2	1	2	2	2			ill									2	1	2	1	2	1	2	1	2	1
73	2	1	1	1	1	1	1	1	2	1	2	1	1	1	2	1	1	1	1	1	2	2	2	2	2	2	2	2	F	1
75	2	2	1	2	6	2	6	6	1	6	1	6	6	1	2	6	1	2	6	2	2	2	2	6	1	2	2	2	2	2
76	F	F									2	6	1	1	1	2	1	1	2	1	2	2	F	2	2	2	2	1	6	6
77	2	6	1	2	6	1	2	6	1	2	1	2	2	1	2	6	6	1	2	2	6	6	6	2	2	2	6	2	2	2
78	1	6	1	2	1	2	1	2	6	6	1	6	1	6	2	1	2	1	2	6	2	2	2	2	1	1	2	1	2	2
79	1	6	1	1	2	1	1	1	1	2	2	1	1	1	2	1	1	1	2	1	2	1	2	1	2	1	2	2	2	2
80	2	2	1	1	1	2	2	2	2	2	2	2	2	1	2	2	2	2	6	1	2	2	6	6	6	6	2	2	6	2
82	2	2	2	2	6	6	6	6	2	2	2	2	2	1	2	6	6	6	1	2	2	2	6	6	6	2	6	1	2	1
83	6	2	1	2	1	1	1	2	2	2	6	2	1	1	1	1	1	1	1	2	2	1	1	1	1	2	2	1	1	1
85	2	2	1	1	2	2	6	1	2	2	2	2	F	F	2	2	1	2	1	2	F	F								
86	1	1	2	1	2	2	1	1	1	2	1	2	2	2	6	1	2	1	2	2	2	2	6	1	2	1	2	2	2	2
87	6	6	2	6	6	2	1	6	1	2	2	2	1	1	2	1	1	2	1	2	2	2	1	2	1	2	1	1	2	1
88	2	2	2	2	2	1	2	2	1	1	6	1	1	1	1	1	1	2	1	2	2	1	2	2	2	2	1	2	2	1
93	2	6	6	2	6	6	1	6	6	1	6	2	2	6	1	2	2	1	1	2	2	2	6	1	2	1	2	1	6	1
95	6	6	1	6	2	2	1	1	2	1	1	2	6	1	2	2	1	6	6	2	2	2	2	6	2	1	2	2	2	6
96	2	2	2	6	1	2	1	1	1	1	1	6	1	6	1	1	1	6	1	1	6	2	2	2	1	2	2	2	2	2
98	2	2	2	2	2	2	2	2	1	2	2	2	2	6	2	1	2	1	2	1	2	2	6	1	1	2	1	2	2	1
100	2	6	2	2	6	1	2	1	1	2	2	1	1	1	2	1	2	2	1	2	2	1	2	1	2	1	2	1	1	1
101	6	6	1	2	6	1	6	1	6	1	6	2	6	1	2	1	2	1	1	2	2	6	2	2	6	1	2	1	2	1
103	1	6	1	1	2	6	1	1	2	6	2	2	1	2	1	1	6	1	1	1	2	6	2	2	6	6	1	1	2	2
104	2	2	2	2	F	2	1	2	2	1	2	2	1	2	1	2	1	6	6	1	2	2	2	2	1	2	6	2	6	2
105	2	6	2	6	1	2	1	2	1	1	2	1	1	6	1	2	1	1	1	1	2	2	2	2	1	2	1	2	1	2
107	2	2	1	2	2	2	2	2	2	2	2	6	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2

TABLE I—*Continued*

Experiment EE.   Response 5 was rewarded

CHICK	TRIALS 1 TO 10										TRIALS 11 TO 20										TRIALS 21 TO 30									
1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
6	—	—	—	—	—	—	—	—	—	—	5	5	5	5	5	5	5	5	5	5										
7	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
10	5	1	6	6	5	5	5	5	5	5	1	5	1	1	1	6	5	1	1	5										
11	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	1	1	1										
12	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
14	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
18	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
19	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
20	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
21	5	5	5	5	5	5	5	5	5	5	5	6	5	5	5	5	6	5	5	5										
23	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
24	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
26	5	5	1	5	1	5	1	5	5	1	5	5	1	1	5	1	5	5	1	5										
27	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5										
29											1	5	5	6	5	5	5	1	5	5										
31	5	5	5	5	5	5	5	5	5	5	5	5		5	5	5	5	5	5	5										
34											1	F	5	5	5	5	5	5	5	5										
37	5	5	5	5	5	5	5	5	5	F	5	5	5	5	5	5	5	5	5	5										
41	5	5	5	5	5	5	1	5	5	5	5	1	5	5	5	5	5	5	1	6										
47	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5											
48	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
49	5	5	5	5	5	5	5	5	5	5	5	5	1	5	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5
50	5	5	5	5	5	5	5	5	5	5	5	1	1	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
51	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
52	1	1	1	1	5	5	5	5	5	5	5	1	1	5	5	5	5	6	5	5	5	5	5	5	5	5	5	5	5	5
53	5	5	5	5	5	5	5	5	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
55	5	5	5	5	5	5	5	5	5	5	6	1	1	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
56	1	5	1	1	1	5	5	5	5	5	1	5	1	5	5	1	5	5	1	5	5	5	5	5	5	5	5	5	5	5
57	5	1	5	5	1	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	1	5	5	5	5
58	1	1	1	1	1	5	5	1	1	1	5	1	5	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5
59	1	5	5	5	5	5	5	5	5	5	5	5	5	5	1	1	1	5	5	5	5	5	5	5	5	5	5	5	5	5
60	5	5	5	5	5	5	5	5	5	5	5	5	1	5	1	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5
61	5	5	5	5	5	5	5	5	5	5	5	5	6	5	5	6	6	5	5	5	5	5	5	5	5	5	5	5	5	5
65	5	5	5	5	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
66	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	1	1	1	1	1	1	5	5	5	5
67	F	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
68	1	6	6	6	6	6	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
69	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
70	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
71	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
73	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	1	5	1	1	5	1	5	1	5	5	5
75	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
76	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
77	5	5	5	5	5	5	5	5	5	5	1	5	5	1	5	5	5	5	5	5	1	5	1	5	5	5	5	5	5	5
78	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
79	5	5	5	5	5	5	5	5	5	5	1	1	6	6	5	5	5	5	5	5	5	5	5	1	5	5	5	5	5	5
80	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	1	5	5	5	5	5	5	5	5	5
82	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
83	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
85	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
86	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
87	5	5	5	5	5	5	5	5	5	5	1	1	1	1	5	5	5	1	5	5	1	1	1	5	5	5	5	5	5	1
88	5	5	5	5	5	5	5	5	5	5	5	1	5	5	5	5	5	1	5	5	5	1	5	5	1	5	5	5	1	1
93	5	5	F	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
95	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
96	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
98	5	5	5	5	6	6	6	6	5	5	1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
100	5	5	5	5	5	5	1	1	1	5	5	5	5	5	5	5	5	5	5	1	1	1	5	5	6	5	5	5	5	
101	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
103	5	5	5	5	5	5	5	5	5		5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
104	F										1	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
105	5	5	5	5	5	5	5	5	5	5	1	5	1	1	5	5	5	5	5	5	1	1	5	5	6	5	5	5	5	5
107	5	5	5	5	5	5	5	5		5	5	5	5	5	5	5	5	5	5	1	1	5	5	1	5	5	5	5	5	

TABLE I—*Concluded*
Experiment FF.   Response 6 was rewarded

CHICK	TRIALS 1 TO 10										TRIALS 11 TO 20										TRIALS 21 TO 30									
1	6	6	6	2	6	6	2	2	2	6	2	2	1	2	1	2	1	1	1	1										
6	2	2	2	2	2	2	2	2	2	2	2	6	2	1	2	2	2	2	2	2										
7	6	6	2	6	6	6	6	6	6	6	1	1	6	2	6	6	2	2	6	6										
10	6	6	6	6	6	6	6	6	6	6	2	2	2	1	6	2	6	2	2	2										
11	1	2	2	2	1	2	1	1	6	6	2	2	2	2	1	1	6	1	2	2										
12	2	1	6	2	6	6	2	6	2	2	6	6	2	2	2	1	2	6	2	6										
14	2	2	6	6	2	6	2	2	2	2	6	2	2	2	1	2	2	1	2	1										
18	2	2	1	2	1	2	2	1	1	2	1	2	2	1	1	1	1	1	1	1										
19	2	2	2	2	2	2	2	2	2	1	2	2	2	2	2	1	1	2	6											
20	6	6	6	6	6	6	6	6	6	6	2	2	2	2	6	1	2	2	2	1										
21	2	F	6	6	6	6	2	6	2	6	2	2	2	2	2	2	2	2	2	2										
23	6	2	2	F	6	6	2	2	2	2	2	2	2	2	1	2	1	2	1											
24	6	6	6	6	6	2	2	2	2	6	2	6	6	6	2	1	2	2	2	6										
26	2	2	2	6	2	6	6	2	2	2	2	2	2	2	2	2	2	2	2	2										
27	2	1	2	2	2	2	2	2	2	2	6	2	6	6	6	6	2	6	6											
29	6	6	2	2	2	2	6	6	2	2	2	2	2	6	2	6	2	2	1	1										
31	F	1	6	6	2	2	6	2	6	6	6	6	6	6	2	2	6	6	2	6										
34	2	2	2	2	2	2	2	1	2	2	2	2	2	2	1	6	2	1	2	1										
37	6	6	6	6	2	2	6	6	2	2	2	6	6	6	6	2	6	6	6											
41	2	F	1	2	1	1	F				2	2	1	F	1	6	2	6	2	2										
47	1	1	1	1	1	1	1	1	1	1	1	2	2	6	6	2	6	2	2	6	6	6	2	6	6	6	6	6	6	6
48	2	2	2	6	6	2	6	6	6	2	2	6	6	6	2	2	6	6	6	2	2	2	6	2	6	2	2	2	6	6
49	2	2	F	1	2	2	2	F	6	2	2	2	2	2	F	2	2	2	F	F	F	6	2	6	2	2	2	2	6	2
50	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	F	2		2	6	2	2	6	2	2	2	6	2
51	1	2	F	2	F	F	1	F			F	F									F	F								
52	6	2	2	6	2	2	6	2	2	2	2	6	2	2	6	6	6	6	6	6	2	2	1	6	6	6	6	6	6	6
53	F	1	2	2	2	1	2	2	2	2	1	6	6	6	2	6	6	6	6	6	6	2	6	6	6	6	6	6	6	6
55	2	1	2	2	2	2	2	2	2	6	2	2	1	2	2	6	6	6	2	2	2	2	2	2	2	1	1	2	2	
56	2	2	2	2	2	2	2	2	2	2	6	2	2	2	6	2	6	2	6	6	2	6	2	6	6	2	2	2	6	6
57	2	2	2	2	2	2	2	1	2	2	2	6	6	6	6	2	2	6	6	2	2	2	6	2	2	6	6	6	6	6
58	2	2	2	2	2	2	2	1	2	6	2	6	6	6	6	6	2	6	6	6	6	6	2	2	2	6	6	6	6	6
59	2	2	2	2	2	2	2	2	1	1	2	6	2	2	2	2	1	2	2	1	F	6	6	2	2	2	2	2	6	6
60	2	2	2	2	F	2	2	2	2	2	2	2	2	6	6	2	2	2	6	6	6	2	6	6	6	2	6	2	1	2
61	6	6	6	6	2	6	6	6	6	2	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	2	6	6
65	2	2	2	2	2	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	2	2	2	2	2	2	2	1	2	2
66	6	6	6	6	6	6	6	6	6	6	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	F	6	2
67	2	2	2	2	2	2	2	2	2	2	6	2	2	2	1	6	2	6	6	2	2	2	2	2	2	1	1	1	6	2
68	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	6	6	2	2	2	2	2	2	6
69	2	2	2	2	6	6	2	2	2	6	2	6	6	6	6	6	6	6	6	6	2	6	2	2	6	2	2	2	6	2
70	6	6	6	6	6	6	6	6	6	6	2	2	6	6	2	2	2	6	6	6	2	6	6	2	2	6	2	1	6	6
71	6	2	6	6	6	6	6	6	6	6	2	2	2	2	2	6	2	6	6	6	2	6	6	6	6	2	1	6	2	6
73	2	2	2	2	2	2	6	6	6	2	2	2	2	1	1	2	6	1	2	1	2	2	2	6	2	1	6	2	2	6
75	6	6	6	6	2	6	6	6	6	6	2	2	2	2	2	2	6	6	6	6	2	2	2	2	6	6	6	6	6	6
76	6	6	6	6	6	6	2	2	2	F	2	6	2	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
77	2	2	2	6	6	6	6	6	6	6	2	6	6	2	6	2	2	6	6	6	2	2	1	2	1	2	1	1	6	6
78	2	2	2	2	2	2	6	2	6	6	6	2	2	2	2	6	6	6	6	6	2	2	2	2	2	2	6	6	2	6
79	6	2	2	6	6	2	6	2	6	6	2	2	2	2	2	2	2	2	6	2	2	2	2	6	2	6	2	6	6	6
80	6	6	6	6	6	6	6	6	6	6	2	2	2	2	2	2	1	1	6	2	1	6	6	6	6	6	1	6	2	
82	2	1	2	6	2	2	2	6	2	2	2	2	2	2	2	1	2	1	F		6	2	2	2	1	1	2	2	6	1
83	6	6	6	2	2	2	2	6	6	6	2	2	2	6	2	2	2	6	2	6	6	6	2	6	6	6	6	6	6	6
85	2	2	2	2	2	2	F	2	2	6	2	2	1	6	1	2	2	1	1	2	6	1	2	1	1	6	6	2	6	6
86	6	6	6	6	6	6	6	6	6	6	2	6	6	2	2	6	6	6	6	2	2	2	2	2	2	2	1	2	2	1
87	6	6	6	6	6	6	6	6	6	6	2	2	2	2	2	F	2	1	2	1	2	2	2	2	2	2	F	2	2	2
88	6	6	2	6	6	2	2	2	2	6	2	2	2	2	F	2	2	F			2	2	2	6	2	6	2	6	2	2
93	6	6	6	2	6	2	6	2	6	6	2	2	2	6	2	6	2	2	6	2	1	1	6	6	6	2	6	6	6	6
95	2	6	6	6	6	6	6	6	6	6	2	6	6	6	6	6	6	6	6	6	2	2	2	6	2	1	2	6	2	6
96	6	6	6	6	6	6	6	6	6	6	6	2	6	2	2	2	2	6	2	2	6	6	2	6	2	6	6	6	6	6
98	6	6	6	2	6	6	6	2	6	6	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	6	1	
100	2	2	2	2	2	2	1	2	1	2	2	2	2	1	2	2	6	2	2	6	2	6	6	2	6	6	2	6	6	6
101	6	6	6	2	6	6	6	6	6	6	2	6	2	2	2	2	6	2	2	2	2	6	2	6	6	2	2	6	6	6
103	6	6	6	F	6	6	6	6	6	6	2	2	2	2	2	2	6	2	2	2	2	2	2	2	2	2	2	6	2	6
104	2	6	2	2	2	F	2	F			2	2	6	2	1	6	6	1	6	2	2	2	1	2	6	1	6	6	6	6
105	6	6	2	6	6	6	6	6	6	6	6	2	2	2	1	1	1	1	6	6	6	6	2	6	6	6	6	6	6	6
107	6	6	6	6	6	2	2	6	6	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2

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trials 1 to 10 of AA, BB, CC, DD, EE, and FF in that order until they were 23 days old, and had trials 11 to 20 in the same order when from 29 days old to 35 days old.[6]

Chicks 48 to 61 began Experiment AA when 13 days old. They had trials 1 to 10 of AA to FF in that order when 13 to 15 days old, trials 11 to 20 (in the same order) when 23 to 27 days old, and trials 21 to 30 (in the same order) when 27 to 29 days old.   They had Experiment G when 33 to 35 days old.

Chicks 65 to 107 began Experiment FF when 24 days old. They had trials 1 to 10 of EE, DD, CC, BB, and AA in that order when from 27 to 34 days old; trials 11 to 20 (in the order FF to AA) when from 36 to 41 days old; and trials 21 to 30 in FF to BB when from 41 days old to 44 days old.   They had Experiment GG when from 45 to 46 days old.

Throughout all the experiments great care was taken to keep conditions identical for punished and rewarded connections in every particular save the punishment or reward itself.   Apart from this, the experimentation was rough and ready.   In partic-ular, the rate of learning might well be different under different conditions.   There was no difficulty in maintaining equality of conditions (other than reward and punishment), except perhaps in maintaining equal standards in the case of CC, DD, and EE as to what constituted a "wrong" response, that is a push or squeeze at the "wrong" door or opening.   The rule was that the head must be pushed definitely into the opening, a mere peck or thrust or close inspection of the opening not being recorded.   Any slow progressive shift of the standard would do no harm to our in-quiry, since it would influence alike those chicks that were having right responses and those that were having wrong responses.

Our first concern with these records is to learn whether in each experiment there was any learning in the sense of a change in the relative frequency of the responses, 1, 2 and 6 (or 1, 5, and 6). Unless there is such a change, it is, of course, idle to measure the comparative influence of a specified reward and a specified punishment in causing it.   The facts appear in Table II.   There

[6] All these ages are assuming that the chicks were hatched on the day reported by the dealer.   Any errors will be slight.

is learning in all six experiments, probably, but it is unquestiona-
ble in experiments BB, CC, and DD, and we shall use these first
and chiefly.   In AA and EE, where the correct response (2 or 5) was
favored from the start, the changes from 1 to 10 and from 11 to 20
were too small to be very reliable or well suited to our problem.
In FF there was slow progress in the first two lots of chicks and
extremely irregular progress in the third lot of chicks, who had
very many more responses of 6 in trial 1 than they had in trial 11.
The result for all was to give the appearance of slow learning from
trial 1 to 10 and very rapid learning from 11 to 20.

In all experiments the interval of seven days or more between
trial 10 and trial 11 reduced the number of right responses greatly,

TABLE II

Number of right responses in trials 1, 10, 11, and 20 for chicks that completed
*twenty trials**

	TRIAL 1	TRIAL 10	TRIAL 11	TRIAL 20
Experiment AA........................	35	43	32	42
Experiment BB........................	18	36	18	42
Experiment CC........................	4	12	6	20
Experiment DD........................	10	17	12	28
Experiment EE........................	53	58	53	59
Experiment FF........................	28	33	10	31

* In case one of the four records was an F, decision concerning whether it should
be assigned to C or X was made on the basis of the neighboring records for the chick
in question.

but this feature of the learning was expected and was desirable
rather than otherwise for our problem.

§4. DETERMINATIONS OF THE INITIAL STRENGTH OF THE
TENDENCIES

There are various methods of analysis which may be applied
to records like those of Table I to measure the effect of a rewarded
"right" connection compared with that of a punished "wrong"
connection.   Those which we shall use require, as a starting-
point, estimates of the initial strength or probable initial fre-
quency of occurrence of BB→1, BB→5, etc., etc.

The initial strength of a chick's tendency to respond by 1, 2, and 6 (or 1, 5, and 6) respectively can be determined approximately by his first trial in the experiment and by preliminary trials in which the consequences are alike for each of the three. It is convenient and economical to have these consequences be the reward of freedom, food, and company for 60 seconds, since that strengthens the tendencies to run and push and squeeze in the direction of the food-pen from which one is to be selected. So each of chicks 48–107 was first given two trials each in AA, BB and FF just as in the experiments described above, except that in every case (1, 2, and 6 or 1, 5, and 6) the act was followed by the lifting of the slide at the food-pen end of the alley, permitting the animal to go out into the food pen, where he was left for 60 seconds or more. These preliminary trials, all rewarded, occurred in a sequence as follows: AA, BB, CC, DD, EE, FF, AA, BB, CC, DD, EE, FF.

The entire sequence was spread over three or more days. In the preliminary trials with CC and DD, all three doors were free to open and in EE all three openings were in soft paper, and the glass slide was lifted if the animal entered any alley by any door or opening. The alleys used were 1, 2, and 6 in AA, DD and EE, and 1, 5, and 6 in BB, CC, and FF.

We have then six choices among 1, 2, and 6, and six choices among 1, 5, and 6 for each animal, from which to compute the probable initial strength of his tendencies to choose 1, 2, and 6 (or 1, 5, and 6). Six would not be enough if there were great individual differences amongst the chicks. We want the number to be as small as will give an adequate picture of the relative frequencies of 1's, 2's, and 6's (or 1's, 5's, and 6's) apart from the rewarding of one and punishing of others, to prevent the fixation of a first accidental choice or of an accidental succession of choices by reward.

In the case of chicks 1 to 47, the preliminary trials were with A, B, C, D, E, and F. After that, these chicks (1 to 47) had AA, BB, CC, etc. with no preliminary trials. The status for chicks 1 to 47 at the beginning of AA, BB, CC, etc. was determined

from their first trials and their previous history in the manner described below and in Appendix I.

Great accuracy must not be expected from these determinations of the initial strength of the tendency of individual chicks to respond by 1 or 2 or 5 or 6. The number of trials is too few. We did not give more because, if enough trials are given (all rewarded, or all punished, or rewarded or punished in some ratio) to get accurate measures of frequency, the tendencies themselves may be changed thereby. The relative strengths at the end point where our learning experiments begin will then not be the same as the relative strengths computed from the entire preliminary series. Such hidden and incalculable constant errors will probably be more harmful than very large variable errors.

What I did was to make the best estimate that I could of each chick's tendencies to respond by 1, 2 and 6 in a 1, 2, 6 choice, and of his tendencies to respond by 1, 5 and 6 in a 1, 5, 6 choice. In making these, I used (for chicks 48 to 107) chiefly the two preliminary trials with AA, BB, CC, DD, EE, and FF in which all responses were equally rewarded, and the first trials of the actual learning.

For chicks 1 to 47 (who lacked the preliminary trials with AA, BB, etc.) I used, instead, the experiments A, B, C, D, E, and F (in which there was always a choice of six locations 1, 2, 3, 4, 5, and 6), and the first trials with AA, BB, CC, DD, EE, and FF. I also attached weight to the average for all chicks in trial 1 of the particular experiment in question. I also used the comparative frequencies of 1 and 2, 1 and 5, 1 and 6, 2 and 5, 2 and 6, and 5 and 6 for all punished trials, as a check. This is permissible because of the evidence presented later proving that a punished occurrence of a connection has little effect on its strength.

The facts from which the estimates of strength of AA→1, AA→2, and AA→6 at the beginning of the AA experiment, of BB→1, BB→5 and BB→6 at the beginning of the BB experiment, and so on, are made, and the ways in which these facts are used, are presented in Appendix I. The final results are the probabilities of Table III.

TABLE III

Estimated initial relative strengths of the tendencies to respond by 1, 2, and 6, and by 1, 5, and 6

CHICK	I PURE INDIVIDUAL			II COMBINED FOR DD			III PURE INDI-VIDUAL			IV COMBINED FOR BB			V COMBINED FOR CC		
	1	2	6	1	2	6	1	5	6	1	5	6	1	5	6
1	8	50	42	10	62	28	9	81	10	13	69	18	13	68	19
6	4	53	43	10	56.5	33.5	18	53	29	16	56	28	16	54	20
7	12	57	31	13	57	30	13	66	21	16	61	23	16	59	25
10	8	42	50	13	50	37	24	50	26	20	51	29	20	50	30
11	27	63	10	22	64	14	17	59	24	22	61	17	22	60	18
12	5	56	39	10	63	27	16	72	12	15	66	19	15	65	20
14	19	66	15	16	71	13	11	81	8	16	75	9	16	73	11
18	38	51	11	34	56	10	38	54	8	36	56	8	36	54	10
19	5	50	45	11	62	27	18	79	3	16	69	15	16	68	16
20	3	69	28	8	74	18	13	85	2	13	77	10	13	76	11
21	5	66	29	13	65	22	29	58	13	22	62	16	22	60	18
23	4	60	36	9	67	24	15	78	7	14	71	15	14	70	16
25	2	67	31	13	65	22	34	54	12	24	60	16	24	59	17
26	12	77	11	21	70	9	45	54	1	32	63	5	32	62	6
27	8	77	15	9	77	14	7	79	14	11	76	13	11	75	14
29	5	49	46	15	56	29	34	58	8	25	57	18	25	56	19
31	4	79	17	18	65	17	47	34	19	32	52	16	32	51	17
34	19	47	34	22.5	56.5	21	34	66	0	29	61	10	29	59	12
37	7	65	28	11	69	20	15	76	9	15	71	14	15	70	15
41	6	74	20	14	71	15	29	64	7	23	67	10	23	66	11
47	25	51	24	20	65	15	13	86	1	19	73	8	19	71	10
48	1	46	53	6	59	35	8	77	15	10	66	24	10	65	25
49	20	64	16	20	69	11	23	76	1	23	71	6	23	70	7
50	20	80	0	20	77	3	24	73	3	23	74	3	24	72	4
51	18	39	43	18	54	28	22	71	7	22	62	16	22	60	18
52	0	50	50	11	57	32	31	60	9	22	58	20	22	57	21
53	26	50	24	21	61	18	13	77	10	19	68	13	19	66	15
55	8	84	8	14	76	10	23	65	12	20	70	10	20	69	11
56	0	61	39	11	63	26	32	60	8	23	61	16	23	60	17
57	13	74	13	23	67	10	50	50	0	36	59	5	36	58	6
58	21	64	15	25	63	12	39	55	6	32	59	9	32	58	10

TABLE III—*Concluded*

CHICK	I PURE INDIVIDUAL			II COMBINED FOR DD			III PURE INDIVIDUAL			IV COMBINED FOR BB			V COMBINED FOR CC		
	1	2	6	1	2	6	1	5	6	1	5	6	1	5	6
59	9	64	27	8	65	27	37	61	2	28	63	9	28	61	11
60	13	55	32	18	60	22	28	63	9	24	61	15	24	60	16
61	4	62	34	8	67	25	13	72	15	13	68	19	13	67	20
65	3	51	46	12	57	31	26	60	14	21	58	21	21	57	22
66	27	66	7	21	70	9	15	76	9	21	71	8	11	70	9
67	1	86	13	5	82	13	8	81	11	10	80	10	10	79	11
68	20	47	33	21	54	25	27	56	17	25	56	19	25	54	21
69	11	49	40	11	62	27	10	79	11	13	69	18	13	68	19
70	0	71	29	6	72	22	11	75	14	11	73	16	11	71	18
71	7	70	23	11	72	17	16	76	8	15	73	12	16	71	13
73	17	75	8	18	72	10	21	66	13	21	68	11	21	67	12
75	13	54	33	14	63	23	14	75	11	16	68	16	16	66	17
76	0	47	53	17	50	33	53	39	8	34	46	20	34	45	21
77	20	56	24	22	60	18	29	61	10	27	60	13	27	59	14
78	10	71	19	13	69	18	17	65	18	17	67	16	17	65	18
79	13	64	23	16	64	20	22	60	18	21	62	17	21	61	18
80	9	61	30	10	68	22	9	79	12	12	72	16	12	70	18
82	7	61	32	11	65	24	17	69	14	16	67	17	16	65	19
83	13	62	25	17	63	20	26	62	12	23	63	14	23	61	16
85	10	82	8	13	75	12	16	63	21	17	69	14	17	67	16
86	28	43	29	23	54	23	17	65	18	22	59	19	22	58	20
87	16	52	32	17	60	23	20	67	13	20	63	17	20	62	18
88	20	72	8	22	71	7	32	66	2	28	67	5	28	66	6
93	7	60	33	8	68	24	8	81	11	11	73	16	11	71	18
95	0	49	51	8	59	33	19	70	11	15	64	21	16	62	22
96	0	67	33	9	67	24	24	64	12	18	65	17	18	64	18
98	7	79	14	12	74	14	21	65	14	18	69	13	19	67	14
100	9	83	8	22	70	8	52	46	2	36	59	5	36	58	6
101	20	53	27	15	65	20	7	85	8	14	73	13	14	71	15
103	7	67	26	9	68	23	8	67	25	11	67	22	11	66	23
104	16	62	22	22	59	19	39	45	16	31	53	16	31	52	17
105	7	68	25	15	67	18	31	62	7	24	64	12	24	63	13
107	7	56	37	8	66	26	7	81	12	11	71	18	11	70	19

§5. METHODS OF COMPARING THE EFFECT OF REWARD WITH THAT OF PUNISHMENT

Having thus an approximate measure of the initial strength of the tendency to choose act 1, 2 or 6 and 1, 5, or 6 for each chick, we may use any or all of the following methods:

A. Let the external situation of presence in the choice chamber be called S. Let the rewarded act be called C and the two punished acts X_1 and X_2. The connections acting in the next trial after a sequence consisting exclusively of C may be compared with their initial strengths; and the connections acting in the next trial after a sequence consisting exclusively of X_1 (or X_2) may be compared with their initial strengths. This could be done keeping both task and animal constant, if each animal had records in enough 1, 2, 6 experiments in which 1 or 2 or 6 was rewarded to obtain percents reliable for that animal. With our data, we can keep only the task constant. Our percents are for groups of animals alike in their initial ↻trengths of the three connections.

B. Let C, X_1 and X_2 mean as before. The connections acting in the next one or two (or three, or more) trials after a sequence rich in C's may be compared with their initial strengths, and similarly after a sequence rich in X_1 (or in X_2). We can with this method use fewer animals (even a single animal) in any one comparison. But we do not have the effect of C or of X_1 or of X_2 pure in these sequences rich in C's or in X_1's or in X_2's as we did by Method A. And we do not use all the data, as we do by Method C.

We have applied Method B to the data, but do not report all the results, since Method C seems preferable.[7]

C. Let C, X_1 and X_2 mean as before. A rough estimate of the average increase in the strength (that is, the percentile frequency) of the connection S→C in any experiment due to any one rewarded occurrence of S→C is made from a general study of the records of that experiment. Call this R. Various estimates of

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[7] The results are in entire harmony with those obtained by Method A and Method C.

the average decrease in strength of $S \rightarrow X_1$ or $S \rightarrow X_2$ due to one punished occurrence of it are made.   Call this P.   For example, P may be 0, or .1R, or .4R, or .7R, or 1.0R or 1.5R.   The first 8 or 10 or 15 or 20 or 25 trials in an experiment may be used for a chick.   We compute the changes in the strength of $S \rightarrow C$, $S \rightarrow X_1$ and $S \rightarrow X_2$ which would result from the addition of 1R to the initial strength of $S \rightarrow C$ in the chick for each occurrence of C, and from the subtraction of 1P from the initial strength of $S \rightarrow X_1$ for each occurrence of X_1 in that chick, and from the subtraction of 1P from the initial strength of $S \rightarrow X_2$ in that chick for every occurrence of X_2.   This is done for each chick separately, and the resulting strengths at the end of trial 8 or 10 or 15 or 20 or 25 by each estimate of the strengthening and weakening effects are compared with the actual strengths as indicated by the frequencies of C, X_1 and X_2 in trials 11 to n, or 16 to n, or 20 to n, or 25 to n.

That ratio of P to R is truest which shows the smallest divergences of the calculated later strengths (in percents) from the actual observed later strengths.

Method C has the advantages of using the data fully, and so of being free from any possible fallacies of selection, of being applicable no matter how slow the learning is, and of using the initial strengths of animals individually.   Its disadvantages are that it is laborious, and that the addition of 1R for each occurrence of $S \rightarrow C$ and the subtraction of 1P for each occurrence of $S \rightarrow X_1$ or $S \rightarrow X_2$ is probably only a very crude approximation to what really happens in the animal's brain.   We have to trust that the ratio of P to R found best by this crude procedure will probably also be best for the net result of probably far more complicated actual effects of rewards and punishments at different stages of the learning.

We have used Method C for all 64 chicks in experiments BB, CC, and DD, except that chicks that failed in any experiment to respond at all by C or X_1 or X_2 in 5 minutes in three or more trials of the first ten or in three or more trials of the second ten were not used in that experiment.

§6. RESULTS FOR EXPERIMENT BB BY METHOD A

I have computed the strengthening influence of rewarded responses and the weakening influence of punished responses in trials 1 to 3 first on the basis of the "combined" estimated initial probabilities, and second on the basis of the "pure individual" estimates.

Using the "combined" estimates of initial status, we find the following for the 18 chicks whose first response was 1 (the rewarded response). Four chicks with initial probabilities of 18 to 21 for 1, averaging 20, showed two responses of 1 in the second trial, or 30 above expectation. Seven cases with initial probabilities of 22 to 25, averaging 23, showed 4 responses of 1 in the second trial (57 percent) or 34 above expectation by chance. Seven cases with initial probabilities of 29 to 36, averaging 33, showed 5 responses of 1 in the second trial (71 percent), or 38 above expectation.

For the eleven chicks whose first two responses were both 1, we have, for similar groupings, 1 as the response in the third trial 2 times out of 2, 2 times out of 4, and 4 times out of 5. The increases above expectation are 80, 27, and 46. For the eight chicks whose first three responses were all 1's, we have for similar groupings the response of 1 in the fourth trial in 1 time out of 2, 2 times out of 2, and 0 times out of 4. The changes from expectation are 29, $76\frac{1}{2}$ and -32.

On a per-occurrence basis our nine determinations give increases of -11, 10, 14, 23, 25.5, 30, 34, 38, and 40, averaging $22\frac{1}{2}$, with a probable error of about 3. If we attach weight to each determination in proportion to the number of occurrences from which it is computed, the average strengthening per occurrence is $+23.5$.[8] If we continue with cases where there are 4 1's in the

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[8] The changes down to 0 and up to 100 should in strictness be treated as *X or less*, and *X or more*, and their inclusion in the determination of average changes introduces an error, which could have been avoided by computing medians instead of averages. They play only an exceedingly small rôle in the weighted averages, however, and the error operates simply to make the averages for the influence of rewarded connections a bit too low and the averages for the influence of punished connections a bit less negative. The use of medians might introduce

first four trials, or 5 1's in the first five trials, the 23.5 will be raised somewhat.

In BB we find the following for the chicks whose first response was 5. 11 chicks with initial probabilities of from 46 to 61, averaging 57, showed 8 responses of 5 in the second trial (73 percent) which is 16 above expectation by chance. 15 chicks with initial probabilities from 63 to 69, averaging 66.4, showed 10 responses of 5 in the second trial (66.7 percent) which is almost exactly at expectation. 15 chicks having initial probabilities from 70 to 77, averaging 73, showed 11 responses of 5 in the second trial (73 percent), or exactly at expectation.

Using the chicks which had two 5's in the first two trials, the numbers and changes from chance expectation are as follows for the groupings just described: 4 5's out of 8, 5 5's out of 10, and 8 5's out of 11, or 7 below chance, 16 below chance, and 0 divergence from chance. Using the chicks with 5 in the first three trials, we have as results for the fourth trial, the following: 2 5's out of 4, 4 5's out of 5, and 6 5's out of 8, or 9 below chance, 13 above chance, and 2 above chance.

Of our nine determinations, three show $-$, three show $+$, and three show 0 (or nearly zero) change from the initial status. Expressing the numbers on a per-occurrence basis, we have -8, -4, -3, 0, 0, $+1$, $+1$, $+4$, and $+16$, giving an unweighted average strengthening of 1 with a probable error of about $1\frac{1}{2}$. The average, weighted according to the number of occurrences involved, is $+0.5$.

Of the four chicks beginning with 6, none responded by 6 in trial 2. Their average initial probability for 6 was 17. So their change from chance expectation for 6 was -17. If we combine this -17 with our results for 5, the resulting average is -1 with a probable error of 2.

The data for BB appear in Table IV.

Using the "pure individual" estimates of initial status in BB, we find the following facts for the eighteen chicks whose first

a worse error, and would certainly make less full use of the data. So it seems best to proceed as I have done.

TABLE IV

Experiment BB. Comparison of the influence of rewarded 1's and that of punished 5's and 6's. Method A. "Combined for BB" estimates of initial probability of occurrence

Group	CC and CX$_1$ and CX$_2$	CC	Per cent CC	Initial probability, C	Change	Group	X$_1$X$_1$ and X$_1$C and X$_1$X$_2$	X$_1$X$_1$	Per cent X$_1$X$_1$	Initial probability, X$_1$	Change	Group	X$_2$X$_2$ and X$_2$C and X$_2$X$_1$	X$_2$X$_2$	Per cent X$_2$X$_2$	Initial probability, X$_2$	Change
I	4	2	50	20	+30	IV	11	8	73	57	+16	VII	4	0	0	1.7	−17
II	7	4	57	23	+34	V	15	10	67	66	+1						
III	7	5	71	33	+38	VI	15	11	73	73	0						

Group	CCC + CCX$_1$ + CCX$_2$	CCC	Per cent CCC	Initial probability, C	Change	Group	X$_1$X$_1$X$_1$ + X$_1$X$_1$C + X$_1$X$_1$X$_2$	X$_1$X$_1$X$_1$	Per cent X$_1$X$_1$X$_1$	Initial probability, X$_1$	Change
Ia	2	2	100	20	+80	IVa	8	4	50	57	−7
IIa	4	2	50	23	+27	Va	10	5	50	66	−16
IIIa	5	4	80	34	+46	VIa	11	8	73	73	0

Group	CCCC + CCCX$_1$ + CCCX$_2$	CCCC	Per cent CCCC	Initial probability, C	Change	Group	X$_1$X$_1$X$_1$X$_1$ + X$_1$X$_1$X$_1$C + X$_1$X$_1$X$_1$X$_2$	X$_1$X$_1$X$_1$X$_1$	Per cent X$_1$X$_1$X$_1$X$_1$	Initial probability, X$_1$	Change
Ib	2	1	50	21	+29	IVb	4	2	50	59	−9
IIb	2	2	100	23.5	+76.5	Vb	5	4	80	67	+13
IIIb	4	0	0	32	−32	VIb	8	6	75	73	+2

Average strengthening per occurrence (unweighted): for C, $22\frac{1}{2} \pm 3$, for X$_1$ or X$_2$, 0 ± 2.

TABLE V

Experiment BB. Comparison of the influence of rewarded 1's with that of punished 5's or 6's. "Pure individual" estimates of initial probability of occurrence

GROUP	CC AND CX₁ AND CX₂	CC	PER CENT CC	INITIAL PROBABILITY, C	CHANGE	GROUP	X₁X₁ AND X₁C AND X₁X₂	X₁X₁	PER CENT X₁X₁	INITIAL PROBABILITY, X₁	CHANGE	GROUP	X₂X₂ AND X₂C AND X₂X₁	X₂X₂	PER CENT X₂X₂	INITIAL PROBABILITY, X₂	CHANGE
A	3	2	67	21	+46	D	5	4	80	49	+31	G	4	0	0	20.5	−20.5
B	5	2	40	23	+17	E	16	10	62.5	66	−3.5						
C	10	7	70	39	+31	F	20	15	75	79	−4						

GROUP	CCC + CCX₁ + CCX₂	CCC	PER CENT CCC	INITIAL PROBABILITY, C	CHANGE	GROUP	X₁X₁X₁ + X₁X₁C + X₁X₁X₂	X₁X₁X₁	PER CENT X₁X₁X₁	INITIAL PROBABILITY, X₁	CHANGE
A1	2	2	100	21	+79	D1	4	1	25	50.5	−25.5
B1	2	2	100	24	+76	E1	10	5	50	66	−16
C1	7	4	57	37	+20	F1	15	11	73	78.5	−5.5

GROUP	CCCC + CCCX₁ + CCCX₂	CCCC	PER CENT CCCC	INITIAL PROBABILITY, C	CHANGE	GROUP	X₁X₁X₁X₁ + X₁X₁X₁C + X₁X₁X₁X₂	X₁X₁X₁X₁	PER CENT X₁X₁X₁X₁	INITIAL PROBABILITY, X₁	CHANGE
A2	2	1	50	21	+29	D2	1	1	100	54	+46
B2	2	2	100	24	+76	E2	5	2	40	65	−25
C2	4	0	0	38	−38	F2	11	9	82	78	+4

response was 1 (the rewarded response). Three chicks with initial probabilities of 21 showed two responses of 1 in the second trial, or 46 above expectation by chance. Five cases with initial probabilities of 22 to 25, averaging 23, showed two responses of 1 in the second trial or 17 above expectation by chance. Ten cases with initial probabilities of 26 or over, averaging 39, showed seven responses of 1 in the second trial, or 31 above expectation.

For the eleven chicks whose first two responses were both 1, two with initial probabilities of 21 had 1 in the third trial in both cases, or 79 above chance expectation. Two with average initial probability of 24 had 1 in the third trial in both cases, or 76 above chance expectation. Seven with an average initial probability of 37 had 1 as the response in trial 3 in four cases, or 57 percent, or 20 above chance.

For the eight chicks whose first three responses were all 1's, we have the following results for the three groups:

> 2 chicks, initial average 21, one 1   in trial 4, or 29 over chance
> 2 chicks, initial average 24, two 1's in trial 4, or 76 over chance
> 4 chicks, initial average 38,   0 1's in trial 4, or 38 below chance

On a per-occurrence basis, these nine determinations give increases over chance of -13, 10, 10, 17, 25, 31, 38, 40, and 46, averaging 23, with a probable error of 4. Weighting in proportion to the occurrences involved, the average is $+16$.

We turn now to the punished responses, beginning with the 41 chicks whose first trial was a 5. We find that 5 chicks with initial probabilities of 60 or less for 5, averaging 49, had 5 as response in trial 2 four times, or 80 percent, or 31 above chance. 16 chicks with initial probabilities of 61 to 72 for 5, averaging 66, had 5 as response in trial 2 10 times, or $62\frac{1}{2}$ percent, or 3.5 below chance. Twenty chicks with initial probabilities for 5 of 73 or over, averaging 79, had 5 as response 15 times in trial 2, or 75 percent, or 4 below chance.

Using the 29 chicks which had two 5's in the first two trials, the numbers and changes from chance expectation are as follows:

> 4 chicks, initial average $50\frac{1}{2}$, had one 5 in trial 3, 25 per cent, or 25.5 below chance

10 chicks, initial average 66, had five 5's in trial 3, 50 per cent, or 16 below chance

15 chicks, initial average 78½, had eleven 5's in trial 3, 73 per cent, or 5½ below chance

Using the 17 chicks which had three 5's in the first three trials, the numbers and changes from chance expectation are as follows:

1 chick, initial 54, had one 5 in trial 4, 100 per cent, +46

5 chicks, initial average 65, had two 5's in trial 4, 40 per cent, −15

11 chicks, initial average 78, had nine 5's in trial 4, 82 per cent, +4

Expressing the changes from chance on a per-occurrence basis, we have −13, −8, −8, −4, −3½, +1, +15, and +31, averaging +1, with a P.E. of 2.

There were only four chicks that responded by 6 in trial 1. None of them responded by 6 in trial 2. Their average initial probability was 20½. So their change from chance expectation was −20½. If we combine this −20½ with the results for chicks responding by 5 in trial 1, the resulting average is a change of −1 with a probable error of 2. Weighting in proportion to the number of occurrences involved, the average is −3.

§7. RESULTS FOR EXPERIMENT BB BY METHOD C

We compute for each chick the frequencies to be expected as a result of the initial frequencies of 1, 5, and 6 plus a strengthening of this or that amount over BB→1 for each occurrence of a rewarded 1 in trials 1 to 15, and a weakening of this or that amount in BB→5 (or BB→6) for each occurrence of a punished 5 (or 6) in trials 1 to 15. We then compare the results of these computations with the actual frequencies found in trials 16 to 20.

The systems of amounts used are 25, 0, 0; 25, −2, −2; 25, −4, −4; 20, 0, 0; and 20, −2, −2;

The computations for chicks 1, 6, and 7 are shown as a sample in Table VI.

The 25, 0, 0, scheme gives very nearly the best fit for frequencies of 1, since by it the minus and plus divergences for 1 are nearly equally frequent (21 −, 31 +, and 2 of 0) and the sums of the plus and minus divergences for 1 are nearly equal (795 − and 802 +). It also is good as regards the divergences for 5.

TABLE VI

Sample of results obtained in comparing the frequencies expected in trial 16 by various schemes of increase in strength for each occurrence of a rewarded connection and decrease in strength for each occurrence of a punished connection

INITIAL STATUS MODIFIED BY ADDITIONS PER OCCURRENCE OF 1, 5, AND 6 IN TRIALS 1 TO 15, OF

CHICK	ESTIMATED INITIAL STATUS			OCCURRENCES IN TRIALS 1 TO 15			OBSERVED FREQUENCY OF OCCURRENCES IN TRIALS 16 TO 20			20, 0, 0			25, 0, 0			20, -2, -2			25, -2, -2			25, -4, -4		
	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6
1	13	69	18	0	12	3	60	20	20	13	69	18	13	69	18	13	45	12	13	45	12	13	21	6
6	16	56	28	1	13	1	60	40		36	56	28	41	56	28	36	30	26	41	30	26	41	4	24
7	16	61	23	4	7	4	40	60		96	61	23	116	61	23	96	47	15	116	47	15	116	33	7

EXPECTED STATUS IN TRIAL 16 BY

CHICK	20, 0, 0			25, 0, 0			20, -2, -2			25, -2, -2			25, -4, -4		
	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6
1	13	69	18	13	69	18	19	64	17	19	64	17	32	53	15
6	36	56	28	41	56	28	39	33	28	42	31	27	59	6	35
7	96	61	23	116	61	23	61	30	9	65	27	8	75	21	4

DIVERGENCES OF EXPECTED STATUS FROM ACTUAL FREQUENCY IN TRIALS 16 TO 20, BY

CHICK	20, 0, 0			25, 0, 0			20, -2, -2			25, -2, -2			25, -4, -4		
	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6
1	-47	49	-2	-47	49	2	-41	44	-3	-41	44	-3	-28	33	-5
6	-30	7	23	-27	5	22	-21	-7	28	-18	-9	27	-1	-34	35
7	13	-26	13	18	-29	11	21	-30	9	25	-33	8	35	-39	4

These number 27 −, 36 +, and 1 of 0, and have as sums 631 for the − and 743 for the + divergences. No schemes otherwise good make a close fit for the 6's, but the 25, 0, 0 scheme is not worse than the others among them. The total sum of divergences is 3680 by 25, 0, 0. It is very nearly as good (3690) by 25, −2, −2, and might be a trifle better by 25, −1, −1; but 25, −4, −4 gives 3781. So the negative effect of a punished 5 or 6 is very near zero by this line of evidence.

It is of course the fact that by using trials 16–20 we have a status reached on the average $2\frac{1}{2}$ trials after the status due to initial status plus trials 1 to 15. If we should compute our expected frequencies with allowance for the effect of trial 16 upon trials 17 to 20, of trial 17 upon trials 18 to 20, of trial 18 upon trials 19 and 20, and of trial 19 upon trial 20, we should have a truer result. I have not done this because it did not seem worth the time and labor. But I have computed the effects of schemes 25, 0, 0, and 25, −2, −2 using trial 16 alone instead of trials 16 to 20. In doing this, I have put together in thirteen groups (twelve of five, and one of four) the records of chicks that had similar initial statuses in BB.

This gives us five (or four for the thirteenth group) records for trial 16, with which to compare the frequencies expected by the 25, 0, 0 and 25, −2, −2 schemes, using for each group the average of the expectations of its members. This procedure is not absolutely equivalent to comparing each individual's expectations with his actual result for trial 16, but is equally just to the two schemes and saves much time. The sum of divergences by 25, 0, 0 is 530; by 25, −2, −2, it is 569. I have made similar computations using trial 17, in which case the sum of divergences is 596 by 25, 0, 0 and 575 by 25, −2, −2. The sum for 16 and 17 together is 1126 by 25, 0, 0, and 1144 by 25, −2, −2.

§8. RESULTS FOR EXPERIMENTS CC AND DD, BY METHOD A

The facts found in CC when the "combined" estimates of initial status are used are reported in Table VII. The unweighted average on a per-occurrence basis is $+31\frac{1}{2}$ (with a probable error of 8) for a rewarded 6, and +11 (with a probable error of 3) for a

TABLE VII

Experiment CC. Comparison of the influence of rewarded 6's and that of punished 1's and 5's. Method A. "Combined for CC" estimates of initial probability of occurrence

	CC→6						CC→5						CC→1				
Group	CC and CX₁ and CX₂	CC	Per cent CC	Initial probability, C	Change	Group	X₁X₁ and X₁C and X₁C₂	X₁X₁	Per cent X₁X₁	Initial probability, X₁	Change	Group	X₂X₂ and X₂C and X₂X₁	X₂X₂	Per cent X₂X₂	Initial probability, X₂	Change
VIII	5	1	20	14	+6	X	5	5	100	55	+45	XIII	10	5	50	20	+30
IX	2	2	100	24	+76	XI	18	12	67	65	+2	XIV	7	3	43	33	+10
						XII	12	5	42	72	−30						
	CCC + CCX₁ + CCX₂	CCC	Per cent CCC				X₁X₁X₁ + X₁X₁C + X₁X₁X₂	X₁X₁X₁	Per cent X₁X₁X₁								
VIIIa	1	1	100	18	+82	Xa	5	4	80	55	+25	XIIIa	5	2	40	18	+22
IXa	2	1	50	24	+26	XIa	12	8	67	66	+1	XIVa	3	2	67	32	+35
						XIIa	5	5	100	73	+27						
	CCCC + CCCX₁ + CCCX₂	CCCC	Per cent CCCC				X₁X₁X₁X₁ + X₁X₁X₁C + X₁X₁X₁X₂	X₁X₁X₁X₁	Per cent X₁X₁X₁X₁								
VIIIb	1	1	100	18	+82	Xb	4	3	75	55	+20	XIIIb	2	2	100	14.5	+85.5
IXb	1	1	100	25	+75	XIb	8	4	50	66	−16	XIVb	2	1	50	30	+20
						XIIb	5	3	60	73	−13						

TABLE VIII

Experiment CC. Comparison of the influence of rewarded 6's and that of punished 1's or 5's. Method A. "Pure individual" estimates of initial probability of occurrence

CC→6

Group	$CC + CX_1 + CX_2$	CC	Per cent CC	Initial probability, C	Change
H	5	1	20	10	+10
I	2	2	100	23	+77

Group	$CCC + CCX_1 + CCX_2$	CCC	Per cent CCC		Change
H1	1	1	100	13	+87
I1	2	1	50	23	+27

Group	$CCCC + CCCX_1 + CCCX_2$	$CCCC$	Per cent CCCC		Change
H2	1	1	100	13	+87
I2	1	1	100	21	+79

CC→5

Group	$X_1X_1 + X_1C + X_1X_2$	X_1X_1	Per cent X_1X_1	Initial probability, X_1	Change
J	5	4	80	56	+24
K	14	9	64	67	−3
L	16	9	56	79	−23

Group	$X_1X_1X_1 + X_1X_1C + X_1X_1X_2$	$X_1X_1X_1$	Per cent $X_1X_1X_1$		Change
J1	4	3	75	55	+20
K1	9	7	78	67	+11
L1	9	7	78	80	−2

Group	$X_1X_1X_1X_1 + X_1X_1X_1C + X_1X_1X_1X_2$	$X_1X_1X_1X_1$	Per cent $X_1X_1X_1X_1$		Change
J2	3	3	100	53	+47
K2	7	3	43	67	−24
L2	7	4	57	80	−23

CC→1

Group	$X_2X_2 + X_2C + X_2X_1$	X_2X_2	Per cent X_2X_2	Initial probability, X_2	Change
M	10	5	50	23	+27
N	7	3	43	44	−1

Group	$X_2X_2X_2 + X_2X_2C + X_2X_2X_1$	$X_2X_2X_2$	Per cent $X_2X_2X_2$		Change
M1	5	1	40	20	+20
N1	3	2	67	45	+22

Group	$X_2X_2X_2X_2 + X_2X_2X_2C + X_2X_2X_2X_1$	$X_2X_2X_2X_2$	Per cent $X_2X_2X_2X_2$		Change
M2	1	1	100	15.5	+84.5
N2	2	1	50	42	+8

TABLE IX

Experiment DD. Comparison of the influence of rewarded 1's and that of punished 2's and 6's. Method A. "Combined for D" estimates of initial probability of occurrence

Group	CC and CX₁ and CX₂	CC	Per cent CC	Initial probability, C	Changes	Group	X₁X₁ and X₁C and X₁C₂	X₁X₁	Per cent X₁X₁	Initial probability, X₁	Change	Group	X₂X₂ and X₂C and X₂X₁	X₂X₂	Per cent X₂X₂	Initial probability, X₂	Change
XV	5	2	40	16	+24	XVII	11	6	55	56	−1						
XVI	5	4	80	21	+59	XVIII	20	13	65	65	0	XX	5	3	6	22	+38
						XIX	14	10	71	73	−2	XXI	3	2	67	31	+36

Group	CCC + CCX₁ + CCX₂	CCC	Per cent CCC	Initial probability, C	Changes	Group	X₁X₁X₁ + X₁X₁C + X₁X₁X₂	X₁X₁X₁	Per cent X₁X₁X₁	Initial probability, X₁	Change	Group	X₂X₂X₂ + X₂X₂C + X₂X₂X₁	X₂X₂X₂	Per cent X₂X₂X₂	Initial probability, X₂	Change
XVa	2	1	50	20	+30	XVIIa	6	1	17	57	−40						
XVIa	4	1	25	23	+2	XVIIIa	13	10	77	65	+12	XXa	3	0	0	23	−23
						XIXa	10	8	80	73	+7	XXIa	2	0	0	33	−33

Group	CCCC + CCCX₁ + CCCX₂	CCCC	Per cent CCCC	Initial probability, C	Changes	Group	X₁X₁X₁X₁ + X₁X₁X₁C + X₁X₁X₁X₂	X₁X₁X₁X₁	Per cent X₁X₁X₁X₁	Initial probability, X₁	Change
XVb	1	1	100	20	+80	XVIIb	1	1	100	59	+41
XVIb	1	0	0	25	−25	XVIIIb	10	6	60	65	−5
						XIXb	8	5	62.5	72	−9.5

TABLE X

Experiment DD.   Comparison of the influence of rewarded 1's and that of punished 2's and 6's.   Method A.   "Pure individual" estimates of initial probability of occurrence

	DD→1						DD→2						DD→6				
Group	CC and CX₁ and CX₂	CC	Per cent CC	Initial probability, C	Changes	Group	X₁X₁ and X₁C and X₁C₂	X₁X₁	Per cent X₁X₁	Initial probability, X₁	Change	Group	X₂X₂ and X₂C and X₂X₁	X₂X₂	Per cent X₂X₂	Initial probability, X₂	Change
O	5	2	40	11	+29	Q	17	9	53	50.5	+2.5						
P	5	4	80	23	+57	R	16	11	69	64	+5	T	5	3	60	30	+30
						S	12	9	75	76	−1	U	3	2	67	46	+21

	DD→1						DD→2						DD→6				
Group	CCC + CCX₁ + CCX₂	CCC	Per cent CCC			Group	X₁X₁X₁ + X₁X₁C + X₁X₁X₂	X₁X₁X₁	Per cent X₁X₁X₁			Group	X₂X₂X₂ + X₂X₂C + X₂X₂X₁	X₂X₂X₂	Per cent X₂X₂X₂		
O1	2	0	0	12.5	−12.5	Q1	9	2	22	51	−29						
P1	4	2	50	22	+28	R1	11	10	91	63	+28	T1	3	0	0	31	−31
						S1	9	7	78	77	+1	U1	2	0	0	47	−47

	DD→1						DD→2					
Group	CCCC + CCCX₁ + CCCX₂	CCCC	Per cent CCCC			Group	X₁X₁X₁X₁ + X₁X₁X₁C + X₁X₁X₁X₂	X₁X₁X₁X₁	Per cent X₁X₁X₁X₁		
P2	2	1	50	20.5	+29.5	Q2	2	2	100	50	+50
						R2	10	6	60	64	−4
						S2	7	4	57	74	−17

punished 1 or 5. The averages, when weighted in proportion to the number of occurrence, are $+25$ and $+5$.

If the pure individual estimates are used, the corresponding facts are as in Table VIII; and the corresponding unweighted averages are $+33$ and $+6$. The corresponding weighted averages are $+25.5$ and $+1$.

In Experiment DD, using the "combined" estimates of initial status, we have the facts of Table IX. The average change (unweighted) per-occurrence is $+20$ (P.E. ±6) for the rewarded 1. For the punished 2, it is 0 (P.E. ±2). For the punished 6 it is $+11$ with a large P.E. For 2 and 6 together, it is $+4$ with a P.E. of 3. If we give weight according to the number of occurrences involved, the effect of a rewarded 1 is $+25$; that of a punished 2 or 6 is 0.

Using the "pure individual" estimates of initial status, we have the facts of Table X. The average change (unweighted) per occurrence is $+21$ (P.E. ±7) for the rewarded 1. For the punished 5 it is $+2$ (P.E. ±2). For the punished 6 it is $+3$ with a large P.E. For 5 and 6 together, it is $2\frac{1}{2}$ with a P.E. of 3. By the weighted averages, we have $+25$ for a rewarded occurrence and $+1$ for a punished.

§9. RESULTS FOR EXPERIMENTS CC AND DD BY METHOD C

The procedure for Experiment CC is in general the same as for BB. We use the records of the 41 chicks that responded by 1, 5, or 6 eighteen or more times. We take the *combined for* CC as the measure of the initial status of each chick. Table XI gives, as samples of the results obtained, those for chicks 1, 11, and 57.

A fairly close fit is obtained by 40, 0, 0 (40 for the rewarded 6, 0 for the punished 1 or 5). By it the sum of the divergences is 2280, distributed as follows: for 1, -387, $+180$; for 5, -474, $+393$; for 6, -279, $+567$. The numbers of $-$ and $+$ divergences are $18 +$ and $23 -$ for the rewarded 6, and $18 +$, $22 -$, and 1 zero for the punished 1, and also for the punished 5. If we should use a little higher or lower credit for a rewarded occurrence (making the scheme say, 45, 0, 0, or 35, 0, 0) we might have a little closer fit. Using 30, 0, 0, however, results in a sum of

TABLE XI

Experiment CC. *Sample of data used in comparing the frequencies expected in trial 16 with the observed frequencies in trials 16 to 20*

CHICK	ESTIMATED INITIAL STATUS			OCCURRENCES IN TRIALS 1 TO 15			OBSERVED FREQUENCY OF OCCURRENCES IN TRIALS 16 TO 20			INITIAL STATUS MODIFIED BY ADDITIONS PER OCCURRENCE OF 1, 5, AND 6 IN TRIALS 1 TO 15, OF											
										0, 0, 40			0, 0, 30			−4, −4, 40			+2, +2, 40		
	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6
1	13	68	19	0	4	11	0	0	100	13	68	458	13	68	348	13	52	458	13	76	458
11	22	60	18	1	8	6	20	20	60	22	60	258	22	60	198	18	28	258	24	76	258
57	36	58	6	6	9	0	60	40	0	36	58	6	36	58	6	12	22	6	48	76	6

CHICK	EXPECTED STATUS IN TRIAL 16 BY												DIVERGENCES OF EXPECTED STATUS FROM ACTUAL FREQUENCY IN TRIALS 16 TO 20, BY											
	0, 0, 40			0, 0, 30			−2, −2, 40			+2, +2, 40			0, 0, 40			0, 0, 30			−2, −2, 40			+2, +2, 40		
	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6
1	2	13	85	3	14	83	3	10	87	2	14	84	2	13	−15	3	14	−17	3	10	−13	2	14	−16
11	6	18	76	8	21	71	6	9	85	7	21	72	−14	−2	16	−12	1	11	−14	−11	25	−13	1	12
57	36	58	6	36	58	6	30	55	15	37	58	5	−24	18	6	−24	18	6	−30	15	15	−23	18	5

2310.   40, −4, −4, results in a sum of divergences of 3086, distributed as follows: for 1, − 512, + 205; for 5, − 900, + 298; for 6, − 131, + 1040.   The distribution of the divergences in 40, 0, 0, 30, 0, 0, and 40, −4, −4, indicates that a better fit will be attained by giving a positive credit for a punished occurrence.

The result by 40, +2, +2, is a sum of divergences of 2208 distributed as follows: for 1, − 350 and + 189; for 5, − 401 and + 451; for 6, − 353 and + 464.   Presumably some larger positive credit for a punished occurrence would produce an even closer fit.

Similar computations are made for DD, using the "combined" estimates of initial status, the effect of trials 1 to 15, and the frequencies in trials 16 to 20 for comparison with the frequencies expected by the application of various amounts of strengthening and weakening.   15, 0, 0 (i.e. 15 for each occurrence of the rewarded 1, and 0 for each occurrence of the punished 1 or 6) gives nearly as close a fit as can be obtained by any k, 0, 0, scheme. The sum of the divergences by it is 3420.   By 20, 0, 0, it is 3464; by 10, 0, 0 it is 3450.   By 15, −1, −1 it is 3452.   By 10, −2, −2, it is 3498.

§10. results for experiments bb, cc, and dd, using average
initial strengths in place of the estimated
initial strengths for individual chicks

We may compare the effect of rewarded and punished occurrences using the assumption that the probable initial relative strength of the tendencies to enter the three alleys was approximately the same for all chicks.   In other words, we may treat our 64 individuals as samples of an average chick at a certain age and inquire how the behavior of an average chick is modified by such and such rewards and punishments.

This method is free from any errors introduced into our previous computations by taking as characteristics of a chick's real initial status some eccentricity which was adventitious.   It also permits us to use the data of trials 1 to 10 and 11 to 20 more fully than we have done, and with freedom from the effect of the long interval between trial 10 and trial 11.   It suffers, of course, from

any errors due to replacing each chick's actual initial status by the average status of all the 64.

I have applied it to Experiments BB, CC and DD. The results for BB are presented in detail; those for CC and DD more summarily. The general outcome is the same appearance of a large strengthening influence for a rewarded connection, and no weakening influence, or a very slight one, for a punished connection.

In Experiment BB the choice was between pathways 1, 5, and 6; 1 being rewarded by freedom, food and company; 5 and 6 being punished by solitary confinement for 30 seconds. In the first trial with BB the frequencies of responses were: 1, 18 or 28 percent; 5, 42 or 66 percent; 6, 4 or 6 percent. In the two preliminary trials with all responses rewarded the frequencies (for the 43 chicks having such) were: 1, 24 or 28 percent; 5, 58 or 67 percent; 6, 4 or 5 percent. We may then safely use 28, 66, and 6 as approximate relative initial strengths for the average chick.

In the 18 cases where the first response was a rewarded 1, the second was 1 much oftener than the expectation by the initial strengths, the frequencies being 11 or 61 percent for 1, 1, and 7 or 39 percent for 1, 5, and 0 for 1, 6. The excess of 1, 1's over expectation is 33. In the 42 cases where the first response was the punished 5, the second was closely according to expectation, the frequency being 29 or 69 percent for 5, 5, and 13 or 31 percent for 5, 1, and 5, 6. There was a slight increase (3) of 5's over expectation. We have only four cases where the first response was the punished 6. It was other than 6 in the second responses of these animals in all four. There was a fall of at least 6 below expectation.

In the 11 cases where the first two responses were both rewarded 1's, the third response was 1 eight times (73 percent) and 5 three times (27 percent). The excess strength of BB→1 over expectation was thus 45. In the 29 cases where the first two responses were both 5's, the third response was 5 eighteen times (62 percent) and 1 or 6 eleven times (38 percent). There was a slight fall (4) from expectation of 5's.

There were no cases of 6, 6 in the first two trials.

In the 7 cases where the first three responses were all 1's, we have 1, 1, 1, 1 three times (43 percent) and 1, 1, 1, 5 four times (57 percent). The increase over expectation is 15 for the BB→1 connection. In the 17 cases where the first three responses were all 5's we have 5, 5, 5, 5 eleven times (65 percent) and 5, 5, 5, 1 or 5, 5, 5, 6 six times (35 percent). The decrease in the strength of the BB→5 connection is 1.

The course of improvement in Experiment B B is complicated by the time interval between trials 1 to 10 and trials 11 to 20. The numbers of rights run as follows for successive pairs of trials:

<div style="text-align:center">36, 33, 40, 48, 64, interval, 40, 47, 70, 81, 82</div>

The number right in the eleventh trial was just the same (18) as in the first. So we may employ as a useful check the learning in trials 11 to 20, starting from the status displayed in trial 11. In it there were 18 1's, 37 5's, 7 6's, and 2 F's. Prorating the 2 F's we have as initial probabilities 29, 60, and 11.

The observed facts in trials 11 and 12 are:

<div style="text-align:center">

1, 1, 12 times (63 per cent); 1, 5, 6 times; 1, 6, once[9]

5, 5, 19 times (51 per cent); 5, 1, 9 times; 5, 6, 9 times

6, 6, 4 times (57 per cent); 6, 1, 2 times; 6, 5, once

</div>

One occurrence of a rewarded 1 thus is followed by an increase in BB→1 of 34. One occurrence of a punished 5 is followed by a decrease in BB→5 of 9. One occurrence of a punished 6 is followed by an apparent increase in BB→6 of 46.

The observed facts in trials 11, 12, and 13 are:

<div style="text-align:center">

1, 1, 1, 5 times (42 per cent); 1, 1, 5, 6 times; 1, 1, 6, once

5, 5, 5, 8 times (42 per cent); 5, 5, 1, 7 times; 5, 5, 6, 4 times

6, 6, 6, once (25 per cent); 6, 6, 1, 2 times; 6, 6, 5, once

</div>

Two occurrences of a rewarded 1 thus are followed by an increase of 13 in BB→1. Two punished 5's are followed by a decrease of 18 in BB→5. Two punished 6's are followed by an increase of 14 in BB→6.

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[9] We have 19 cases, because we use the sequence F, 1, 1. We also use the sequence 5, F, 5.

The observed facts in trials 11 to 14 are: 1 1 1 1, 5 times (100 percent). This is 72 above chance. 5 5 5 5, 6 times (75 percent); 5 5 5 1, once; 5 5 5 6, once. BB→5 is thus 9 above chance. 6 6 6 1, once (100 percent). This is 94 above chance. All these results are summarized in column 2 of table XII.

We may use Method B and enrich our material at the expense of absolute purity of C or X_1 or X_2 by taking the cases where the first eight are rich in 1, or rich in 5, or rich in 6, and observing the consequences in the case of trial nine. We may add to the reliability of the results at the expense of measuring the consequences of trials 1 to 8 plus some influence from trial 9, by using the consequences in the case of 9 and 10. The six cases richest in 1's in trials 1 to 8 had 44 of them and 4 5's. In trials 9 and 10, they had 10 1's and 2 5's, i.e. 55 increase above expectation for BB→1. The next richest six had 34 1's, 12 5's, and 2 6's in trials 1 to 8, and had 9 1's and 3 5's in trials 9 and 10, or 47 increase above expectation for BB→1.

The eight cases richest in 5's had 57 5's, 5 6's, a single 1, and 1 F in trials 1 to 8. In trials 9 and 10 they had 10 5's ($62\frac{1}{2}$ percent), 4 1's (25 percent), and a 6 ($12\frac{1}{2}$ percent), i.e., $3\frac{1}{2}$ below expectation for BB→5. The twelve cases next richest in 5's had 72 5's, 11 1's, 10 6's and 3 F's in trials 1 to 8. In trials 9 and 10 they had 8 5's (33 percent), 12 1's (50 percent), and 4 6's (17 percent), i.e., 33 below the expectation for BB→5. This is the first evidence of any appreciable effect of punishment in the way of weakening the punished connection. It is itself ambiguous, since the rewarded 1's may have caused part of it.

Using trials 11 to 18 and trials 19 and 20 in this same way, we have the following results:

The 6 cases richest in 1's had 45 1's, 2 5's, and 1 F in trials 11 to 18. They had 12 1's, 100 percent, in trials 19 and 20. The increase in BB→1 was thus 71. Each of the ten cases next richest in 1's had 6 1's, making 60 in all, with 12 5's and 8 6's, in trials 11 to 18. In trials 19 and 20, they had 14 1's, (70 percent), 4 5's and 2 6's. The increase was thus 41 for them.

The 9 cases richest in 5's had 55 5's, 13 1's, and 4 6's in trials 11 to 18. In trials 19 and 20 they had 11 5's (61 percent or an

increase of $2\frac{1}{2}$) and 7 1's. The 4 cases next richest in 5's had 20 5's, 5 1's, 6 6's, and 1 F in trials 11 to 18. In trials 19 and 10 they had 5 5's ($62\frac{1}{2}$ percent, or an increase of $2\frac{1}{2}$) and 3 1's. These also thus showed an apparent slight strengthening of BB→5.

In Experiment CC the choice was between pushing under top-hung doors 1, 5, and 6; a strong or persistent push under 6 was rewarded; a push under 1 or 5 was punished by thwarting.   The initial strengths of CC→1, CC→5 and CC→6 for the average chick may be set as 28, 59, and 13 per hundred.   The frequencies for the first trial were (in percents) 28, 59, and 13 for 1, 5, and 6 respectively.   The frequencies in two preliminary trials in which all three doors could be pushed back and in which pushing under any one of them was rewarded, were (in percents) 9, 86, and 5 respectively.   Consequently the 28 and 13 which we use are, if in error at all, probably too high; and the 59, too low.

In the 9 cases in which the first response was 6,[10] the next response was 6 in 67 percent or 54 over the estimated initial strength.   In the 35 cases where the first trial resulted in a 5, the next response was 5 in 63 percent (4 above initial strength), 1 in 20 percent, and 6 in 17 percent.   There is no weakening of BB→5.   In the 15 cases where the first trial resulted in a 1, the next response was 1 in $46\frac{2}{3}$ percent (19 above initial strength), 5 in $46\frac{2}{3}$ and 6 in $6\frac{2}{3}$ percent.   There is no weakening of BB→1.

These and other consequences of C's and X's, computed in the same way as those for BB, appear in Table XII, column 4.

We next study the sequents of the first, first two, and first three responses in trials 11 to 20.   These furnish a useful check in CC as they did in BB, since the interval between trial 10 and trial 11 brought the frequencies of the three connections back nearly to their initial status.   The numbers correct for the 40 chicks who had twenty trials or more were in order, 4, 7, 5, 12, 5, 5, 12, 12, 17, 12, interval 6, 8, 10, 14, 15, 13, 15, 21, 20, and 20.   The frequencies in trial 11 were 10, 75, and 15 for 1, 5, and 6 respectively.

Using 10, 75 and 15 as strengths for 1, 5, and 6 respectively at

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[10] Here, as elsewhere in the use of Method A, trials which resulted in F's are simply passed over, F, 6, 6, or 6, F, 6 being treated as 6, 6.

trial 11, the consequences of C's and X's, computed as before, appear in Table XII, column 10.

We next observe the cases where trials 1–8 were rich in the rewarded CC→6, and those where trials 1–8 were rich in punished CC→1 or CC→5. We do not use those chicks (Nos. 10, 12, 14, 18, 21, 23, 26, 27, 34, 41, 47, 53, 56, 57, 58, 66, 67, 68, 76, 78, 80, 83, 98, 104) that did not have trials 9 and 10 by reason of earlier failures, or that failed in both 9 and 10.   That leaves 47 cases.

We find that the occurrence of many rewarded 6's strengthened the CC→6 connection with such aid as a very few punished 1's and 5's may give, but that the occurrence of many punished 1's or 5's did not weaken the CC→1 or CC→5 connection more than can probably be accounted for by the rewarded 6's that are involved.   Thus the 3 chicks richest in 6's in trials 1–8 had in trials 1–8 22 6's, 1 5 and 1 F.   In 9 and 10 they had 100 percent 6's or 87 over initial strength.   The 4 chicks next richest in 6's had 24 6's, 4 1's, 2 5's and 2 F's.   In 9 and 10 they had $87\frac{1}{2}$ percent 6's or $64\frac{1}{2}$ above initial strength.

The 9 chicks richest in 5's, with 65 5's, 6 6's and a single 1 in trials 1 to 8, had 89 percent 5's in 9 and 10.   The 3 chicks richest in 1's (21 1's, 2 5's, and 1 6) did show a decrease in CC→1 in 9 and 10, to 16.7.   But the increase of those richest in 5's more than balances this.   The next richest group in 5's and 1's also showed an increase.   Six chicks with 36 5's, 5 1's and 7 6's in 1 to 8, had 75 percent of 5's in 9 and 10.   4 chicks having 20 1's 7 5's, and 5 6's in 1 to 8, had 25 percent of 1's in trials 9 and 10. 9 chicks having 45 5's, 14 1's, 11 6's and 2 F's in 1 to 8, had 56 percent 5's, or only 3 below initial strength, in trials 9 and 10.

Similar facts are found for trials 11 to 18.   The 9 chicks richest in 6's in 11–18 (57 6's, 5 1's, and 10 5's) had 100 percent 6's in 19 and 20, or 85 increase over the strength in trial 11.   The 10 chicks richest in 5's in 11–18 (70 5's, 4 1's and 6 6's) had 65 percent of 5's in 19 and 20.   The 2 chicks richest in 1's in trials 11–18 (11 1's, 4 5's, and 1 6) had 50 percent of 1's in trials 19 and 20.   The total for the 11 chicks is a frequency for CC→5 or CC→1 reduced only a little below the frequency in trial 11.

The first responses to DD for the 62 chicks that had only one F in trials 1 to 10 numbered 9 for 1, 43 for 2, 8 for 6, and 2 for F. One of the F's was followed in trial 2 by a 1 and one other by a 2. So we set the initial status of the average chick in DD as 16, 71 and 13. Computations similar to those made for BB and CC give the results appearing in Table XII, column 6.

In trial 11, the frequencies were 11 for 1, 38 for 2, 10 for 6, and 4 F's (one of which was followed by a 1 and another of which was followed by a 6), and one 2 followed by F's thereafter. So we

TABLE XII

Comparison of the effect of rewarded occurrences with that of punished occurrences, using the average initial strength in place of estimated strengths in individual chicks. Changes from expectation by chance

Ch. = change. N. = number of chicks involved in the determination.

	IN TRIALS 1 TO 3						IN TRIALS 11 TO 13					
	BB		CC		DD		BB		CC		DD	
	Ch.	N.	Ch.	N.	Ch.	N.	Ch.	N.	Ch.	N.	Ch.	N.
1	2	3	4	5	6	7	8	9	10	11	12	13
CC	+33	*(18)*	+54	*(9)*	+44	*(10)*	+34	*(19)*	+35	*(6)*	+14	*(12)*
CCC	+45	*(11)*	+54	*(6)*	+17	*(6)*	+13	*(12)*	+85	*(1)*	+55	*(4)*
CCCC	−15	*(7)*	+87	*(4)*	+34	*(2)*	+71	*(5)*	+85	*(1)*	+80	*(3)*
X_1X_1	+3	*(42)*	+4	*(35)*	−7	*(44)*	−9	*(37)*	+3	*(27)*	−15	*(47)*
$X_1X_1X_1$	−4	*(29)*	+22	*(22)*	−3	*(28)*	−18	*(19)*	+5	*(20)*	−6	*(18)*
$X_1X_1X_1X_1$	−1	*(17)*	+6	*(17)*	−9	*(19)*	+15	*(8)*	+2	*(15)*	−12	*(10)*
X_2X_2	−6	*(4)*	+19	*(15)*	+49	*(8)*	+46	*(7)*	+65	*(4)*	+9	*(11)*
$X_2X_2X_2$			+29	*(7)*	−13	*(6)*	+14	*(4)*	+23	*(3)*	−18	*(3)*
$X_2X_2X_2X_2$			+47	*(4)*			−11	*(1)*	−10	*(1)*		

set the status at trial 11 for the average chick at 20, 62, and 18. Computations similar to those made for BB and CC give the results appearing in Table XII, column 12.

The unweighted averages (more properly, the averages with equal weight for each determination of Table XII) are +25 per rewarded occurrence in trials 1, 2 and 3, +26 per rewarded occurrence in trials 11, 12, and 13, +7 per punished occurrence in trials 1, 2, and 3, and $+5\frac{1}{2}$ per punished occurrence in trials 11, 12, and 13.

If we attach weight to each determination in proportion to the number of occurrences the effect of which is measured, we have the following:

In BB, +18 for C (+16 in 1, 2, 3, and +20 in 11, 12, 13)
In BB, −1 for X (0 in 1, 2, 3, and − 1½ in 11, 12, 13)
In CC, +35 for C (+35 for 1, 2, 3, and +35 for 11, 12, 13)
In CC, +7 for X (+9½ for 1, 2, 3, and +4½ for 11, 12, 13)
In DD, +22 for C (+22 for 1, 2, 3, and +22 for 11, 12, 13)
In DD, −4 for X (−1 for 1, 2, 3, and −7 for 11, 12, 13)

In applying Method C to the records with the use of the average initial status, we study first the prophecy of trials 9 and 10 from trials 1 to 8, and of trials 19 and 20 from trials 11 to 18. We group the chicks into four or more groups as nearly alike as may be in the distribution of C, X_1 and X_2 in the eight learning trials. Thus Group A in Table XIII was constituted of chicks that had many 1's in BB; Group E was constituted of chicks that had many 5's in BB.   The comparisons for each of these groups are made just as they were for individual chicks in Tables VI and XI. Table XIII shows the comparisons between schemes 30, 0, 0 and 30, −2, −2.   Table XIV shows the results by schemes 20, 0, 0; 20, −2, −2; and 30, −4, −4.

In the case of BB, we compute the expected frequencies in trials 9 and 10 (and 19 and 20) from the observed frequencies in trials 1 to 8 (and 11 to 18) in five ways:

(1) Adding +20 to the initial 28 for the effect of each occurrence of BB→1 in trials 1 to 8, and adding nothing to the initial 66 and 6 for an occurrence of BB→5 or BB→6, we have the facts of columns 3, 4, 5, and 6 of Table XIV, rows A to E.   Adding 20, 0, 0 similarly to the 29, 60, and 11 of the 11th trial for each occurrence of 1, 5, and 6 respectively in trials 11 to 18, we have the facts of rows F to K of columns 3, 4, 5, and 6 of Table XIV. Combining both, we have a divergence of expected frequencies from actual frequencies in trials 19 and 20 of 237.8.

(2) Using 30, 0, and 0 as the additions per occurrence (of 1, 5, and 6, respectively) we have the results shown in the appropriate columns and rows of Table XIII.   Combining we have a divergence of expected frequencies from actual frequencies of 212.6.

TABLE XIII

Experiment BB. Comparison of Scheme 30, 0, 0 and Scheme 30, −2, −2. By groups

GROUP	N	ACTUAL FREQUENCIES Averages (In 1 to 8 / In 11 to 18)			ACTUAL In per cents (In 9 and 10 / In 19 and 20)			EXPECTED By 30, 0, 0			EXPECTED By 30, −2, −2			DIVERGENCES By 30, 0, 0			DIVERGENCES By 30, −2, −2			SUM OF DIVERGENCES By 30,0,0	By 30,−2,−2
		1	5	6	1	5	6	1	5	6	1	5	6	1	5	6	1	5	6		
A	10	6.8	1.1	0.1	75.0	25.0	0.0	76.3	21.7	2.0	76.9	21.2	1.9	1.3	−3.3	2.0	1.9	−3.8	1.9	6.6	7.6
B	11	3.9	3.7	0.3	59.1	40.9	0.0	66.8	30.4	2.8	69.4	28.0	2.6	7.7	−10.5	2.8	10.3	−12.9	2.6	21.0	25.8
C	14	2.0	4.6	1.1	50.0	39.3	10.7	55.0	41.3	3.7	59.2	38.2	2.6	5.0	2.0	−7.0	9.2	−1.1	−8.1	14.0	18.4
D	15	1.0	5.1	1.6	48.3	31.0	20.7	44.6	50.8	4.6	49.8	47.8	2.4	−3.7	19.8	−16.1	1.5	16.8	−18.3	39.6	36.6
E	11	0.0	6.5	1.3	31.8	54.6	13.6	28.6	66.0	6.0	33.2	62.8	4.0	−3.8	11.4	−7.6	1.4	7.2	−9.6	22.8	19.2
F	16	6.6	0.9	0.5	81.3	12.5	6.2	76.2	20.1	3.7	76.9	19.7	3.4	−5.1	7.6	−2.5	−4.4	7.2	−2.8	15.2	14.4
G	9	5.0	1.2	1.6	77.8	22.2	0.0	71.6	24.0	4.4	73.3	23.6	3.1	−6.2	1.8	4.4	−4.5	4.4	3.1	12.4	9.0
H	10	4.0	2.9	1.0	75.0	20.0	5.0	67.7	27.3	5.0	70.3	25.5	4.2	−7.3	7.3	0.0	−4.7	5.5	−0.8	14.6	11.0
I	7	3.0	3.7	1.3	57.2	35.7	7.1	62.6	31.6	5.8	66.1	29.2	4.7	5.4	−4.1	−1.3	8.9	−6.5	−2.4	10.8	17.8
J	12	2.0	4.3	1.6	54.2	45.8	0.0	55.6	37.5	6.9	60.1	34.7	5.2	1.4	−8.3	6.9	5.9	−11.1	5.2	16.6	22.2
K	9	0.7	5.2	2.0	22.2	66.7	11.1	41.7	50.0	8.3	44.6	46.9	8.5	19.5	−16.7	−2.8	22.4	−19.8	−2.6	39.0	44.8
Sum of minus divergences														26.1	42.9	37.3	13.6	55.2	43.6		
Sum of plus divergences														30.3	49.9	16.1	61.5	38.1	12.8		
Total																				212.6	226.8

(3) Using 20, −2, and −2 as the additions per occurrence of 1, 5, and 6, respectively, we have the results shown in columns 7, 8, 9, and 10 of Table XIV.   Combining we have a divergence of expected frequencies from actual frequencies of 229.8.

(4) Using 30, −2, and −2 as the additions per occurrence of 1, 5, and 6, respectively, we have the facts of the appropriate

TABLE XIV

Experiment BB.   Results of the application of Schemes 20, 0, 0, 20, −2, −2, and 30, −4, −4.   By groups, the same as in Table XIII

1	2	3	4	5	6	7	8	9	10	11	12	13	14
		DIVERGENCES OF EXPECTED FROM ACTUAL FREQUENCIES											
GROUP	N	By 20, 0, 0				By 20, −2, −2				By 30, −4, −4			
		In 9 and 10				In 9 and 10				In 9 and 10			
		1	5	6	Sum	1	5	6	Sum	1	5	6	Sum
A	10	−5.5	3.0	2.5	11.0	−4.8	2.3	2.5	9.6	2.5	−4.4	1.9	8.8
B	11	0.4	−3.8	3.4	7.6	3.2	−6.4	3.2	12.6	13.0	−15.4	2.4	30.8
C	14	−1.4	7.8	6.4	15.6	2.8	4.9	−7.7	15.4	14.1	−4.6	−8.5	28.2
D	15	−8.3	24.0	−15.7	48.0	−3.3	21.3	−18.0	42.6	7.6	13.1	−20.7	41.4
E	11	−3.8	11.4	−7.6	22.8	1.4	8.2	−9.6	19.2	8.9	3.5	−12.4	24.8
		In 19 and 20				In 19 and 20				In 19 and 20			
		1	5	6	Sum	1	5	6	Sum	1	5	6	Sum
F	16	−11.9	13.3	−1.4	26.6	−11.1	12.9	−1.8	25.8	−3.6	6.8	−3.2	13.6
G	9	−13.5	8.0	5.5	27.0	−11.4	7.4	4.0	22.8	−2.8	0.9	1.9	5.6
H	10	−14.4	13.3	1.1	28.8	−11.7	11.5	0.2	23.4	−2.1	3.7	−1.6	7.4
I	7	−1.6	1.8	−0.2	3.6	2.1	−0.6	−1.5	4.2	12.8	−9.1	−3.7	25.6
J	12	−4.9	−2.9	7.8	15.6	−0.4	−5.7	6.1	12.2	11.1	−14.4	3.3	28.8
K	9	15.6	−14.1	−1.5	31.2	21.0	−16.9	−4.1	42.0	32.0	−24.2	−7.8	64.0
					237.8				229.8				279.0

columns and rows of Table XIII.   Combining, the divergence of expected frequencies from actual frequencies is 226.8.

(5) Using 30, −4, and −4, we have the facts of columns 11, 12, 13, and 14 of Table XIV, the combined divergences being 279.

Examination of the details of Tables XIII and XIV will show that the fit obtained by the use of 30, 0, and 0 is nearly as good as that obtainable by any one system of the k, 0, and 0 type.

TABLE XV

Experiment CC. Results of the application of Schemes 60, 0, 0, 60, –4, –4, and 60, +1, +1. By groups

(The 60 is applied to 6's; the 0 or –4 or +1, to 1's and 5's)

GROUP	N	AVERAGE FREQUENCIES In 1 to 8			ACTUAL FREQUENCIES IN 9 AND 10, IN PER CENTS			DIVERGENCES OF ACTUAL FROM EXPECTED FREQUENCIES IN 9 AND 10 By 60, 0, 0			By 60, –4, –4			By 60, +1, +1			SUMS OF DIVERGENCES By 60, 0, 0	By 60, –4, –4	By 60, +1, +1
		1	5	6	1	5	6	1	5	6	1	5	6	1	5	6			
I	8	0.5	0.9	6.3	6.3	6.2	87.5	–0.4	6.1	–5.7	–0.8	5.5	–4.7	–0.4	6.3	–5.9	12.2	11.0	12.6
II	8	1.1	4.1	2.4	6.2	37.5	56.3	5.3	–13.3	8.0	4.4	–18.4	14.0	5.5	–12.2	6.7	26.6	36.8	24.4
III	14	0.8	6.1	0.9	3.6	75.0	21.4	14.6	–36.7	22.1	16.0	–47.6	31.6	14.3	–34.6	20.3	73.4	95.2	69.2
IV	6	6.0	1.5	0.5	25.0	33.3	41.7	–3.5	12.1	–8.6	–21.0	19.7	1.3	–0.3	10.7	–10.4	24.2	42.0	21.4
V	12	2.4	5.3	0.0	13.0	69.6	17.4	15.0	–10.6	–4.4	14.5	–12.5	–2.0	15.3	–9.9	–5.4	30.0	29.0	30.6

GROUP	N	AVERAGE FREQUENCIES In 11 to 18			ACTUAL FREQUENCIES In 19 and 20			DIVERGENCES OF ACTUAL FROM EXPECTED IN 19 AND 20 By 60, 0, 0			By 60, –4, –4			By 60, +1, +1			By 60, 0, 0	By 60, –4, –4	By 60, +1, +1
		1	5	6	1	5	6	1	5	6	1	5	6	1	5	6			
VI	9	0.6	1.1	6.3	0.0	0.0	100.0	2.1	15.7	–17.8	1.6	15.0	–16.6	2.2	15.9	–18.1	35.6	33.2	36.2
VII	8	0.4	4.4	3.2	12.5	18.8	68.7	–9.1	6.9	2.2	–9.4	2.2	7.2	–9.0	7.9	1.1	18.2	18.8	18.0
VIII	12	1.6	5.1	1.3	12.3	33.3	54.2	–6.9	8.8	–1.9	–10.1	2.8	7.3	–6.2	10.1	–3.9	17.6	20.2	20.2
IX	8	1.9	6.1	0.0	6.2	81.3	12.5	3.8	–6.3	2.5	–2.7	–6.9	9.6	4.8	–6.2	1.4	12.6	19.2	12.4
Sum of minus divergences								–19.9	–66.9	–38.4							250.4	305.4	245.0
Sum of plus divergences								40.8	49.6	34.8							Total........		

TABLE XVI

Experiment CC. Results of the application of Schemes 40, 0, 0, 30, 0, 0, and 30, +4, +4. By groups

DIVERGENCES OF EXPECTED FROM ACTUAL FREQUENCIES

GROUP	N	By 40, 0, 0				By 30, 0, 0				By 60, +4, +4			
		1	5	6	Sum	1	5	6	Sum	1	5	6	Sum
		In 9 and 10				In 9 and 10				In 9 and 10			
I	8	1.6	10.6	−12.2	24.2	3.4	14.2	−17.6	35.2	−0.1	6.8	−6.7	13.6
II	8	8.1	−7.4	−0.7	16.2	10.1	−3.2	−6.9	20.2	6.0	−9.0	3.0	18.0
III	14	17.0	−31.6	14.6	63.2	18.4	−28.5	10.1	57.0	13.6	−29.1	15.5	58.2
IV	6	−1.7	15.9	−14.2	31.8	−0.7	20.8	−21.5	43.0	7.5	7.3	−14.8	29.6
V	12	15.0	−10.6	−4.4	30.0	15.0	−10.6	−4.4	30.0	15.8	−8.3	−7.5	31.6
		In 19 and 20				In 19 and 20				In 19 and 20			
VI	9	2.8	21.3	−24.1	48.2	3.5	25.9	−29.4	58.8	2.6	16.4	−19.0	38.0
VII	8	−8.1	14.1	−6.0	28.2	−7.4	19.5	−12.1	39.0	−8.8	11.0	−2.2	22.0
VIII	12	−5.9	16.0	−10.1	32.0	−5.3	20.7	−15.4	41.4	−4.5	13.3	−8.8	26.6
IX	8	3.8	−6.3	2.5	12.6	3.8	−6.3	2.5	12.6	7.1	−6.0	−1.1	14.2
Sum					286.4				337.2				251.8

The details of the tables also show that assigning zero effect to a punished occurrence gives nearly or quite as good a fit as the assignment of any negative effect whatsoever, and that if any negative effect is better than zero, it is a very small one compared to the positive effect of a rewarded connection.

A better fit could be obtained by having one system of additions for trials one to eight and a difference system for trials 11 to 18. But this is unimportant for our general purpose.

The procedure in the case of CC is the same as that used with the data of Experiment BB, except that there are enough cases in CC that were rich in 1's in trials 1 to 8 to make a separate group. The facts presented in Tables XV and XVI (which are of the same plan as Tables XIII and XIV) show that additions of 60, 0, and 0 per occurrence of CC→6, CC→1 and CC→5 respectively will give the best fit of any k, 0, 0 scheme, and that no allowance for weakening by the punishment is desirable.

The 60, 0, 0 scheme produces a divergence of expected occurrences of 6, 1 and 5 in trials 9 and 10 and 19 and 20 from the actual occurrences of 250 (250.4), and gives almost equal plus and minus divergences for 6 (+34.8 and −38.4), and for 1 and 5 combined (+90.4 and −86.8).

Using 60, −4, −4 sends the sum of divergences up to 305. Indeed, there is some evidence that a punished CC→1 or CC→5 gains more strength by occurring than it loses by being punished. A scheme of +60, +1, +1, produces a slightly better fit than 60, 0, 0 does, (245.0 versus 250.4), and a scheme of 60, +4, +4, produces nearly as good a fit as 60, 0, 0 (251.8). A scheme of about 62, +2, +2, will probably give the best fit of any such simple scheme of uniform additions for any rewarded 6 and uniform additions or subtractions for punished 1's and 5's.

The records of DD are treated in the same way as those of BB and CC. Table XVII shows that 15, 0, 0, gives a closer fit than 15, −1, −1. From the details of Table XVII it may be inferred that 15, −2, −2, will be worse, and that some scheme very near to 15, 0, 0 will give the best fit.

TABLE XVII

Experiment DD. Comparison of Scheme 15, 0, 0 and Scheme 15, −1, −1. By groups

GROUP	N	AVERAGE FREQUENCIES In 1 to 8			ACTUAL FREQUENCIES In 9 and 10			EXPECTED FREQUENCIES By 15, 0, 0 In 9 and 10			EXPECTED FREQUENCIES By 15, −1, −1 In 9 and 10			DIVERGENCES By 15, 0, 0			DIVERGENCES By 15, −1 −1			SUM OF DIVERGENCES By 15, 0, 0	SUM OF DIVERGENCES By 15, −1
		1	2	6	1	2	6	1	2	6	1	2	6	1	2	6	1	2	6		
A	7	5.4	1.6	1.0	50.0	42.9	7.1	53.6	39.2	7.2	54.3	38.9	6.8	3.6	−3.7	0.1	4.3	−4.0	−0.3	7.4	8.6
B	11	3.2	4.8	0.0	22.7	68.2	9.1	43.2	48.0	8.8	44.7	46.2	9.1	20.5	−20.2	−0.3	22.0	−22.0	0.0	41.0	44.0
C	11	2.7	2.7	2.2	40.9	31.8	27.3	40.0	50.7	9.3	41.5	50.5	8.0	−0.9	18.9	−18.0	−0.6	−18.7	19.3	37.8	38.6
D	14	0.4	4.4	3.1	21.4	57.2	21.4	20.8	67.0	12.2	22.2	67.6	10.1	−0.6	9.8	−9.2	0.9	10.4	−11.3	19.6	22.6
E	7	0.4	1.7	5.4	33.3	16.7	50.0	20.8	67.0	12.2	22.2	70.1	7.7	−12.5	50.3	−37.8	−11.1	53.4	−42.3	100.6	106.8
F	11	0.5	7.2	0.2	27.3	72.7	0.0	21.9	66.0	12.1	23.5	63.7	12.8	−5.4	−6.7	12.1	−3.8	−9.0	12.8	24.2	25.6

GROUP	N	AVERAGE FREQUENCIES In 11 to 18			ACTUAL FREQUENCIES In 19 and 20			EXPECTED FREQUENCIES By 15, 0, 0 In 19 and 20			EXPECTED FREQUENCIES By 15, −1, −1 In 19 and 20			DIVERGENCES By 15, 0, 0 In 19 and 20			DIVERGENCES By 15, −1 −1 In 19 and 20			SUM OF DIVERGENCES By 15, 0, 0	SUM OF DIVERGENCES By 15, −1
		1	2	6	1	2	6	1.	2	6	1	2	6	1	2	6	1	2	6		
G	11	5.6	1.8	0.6	81.8	18.2	0.0	56.5	33.7	9.8	57.3	33.2	9.5	−25.3	15.5	9.8	−24.5	15.0	9.5	50.6	49.0
H	14	3.6	4.1	0.1	64.3	35.7	0.0	48.0	40.3	11.7	49.4	38.6	12.0	−16.3	4.6	11.7	−14.9	2.9	12.0	32.6	29.8
I	9	3.0	3.2	1.8	33.3	38.9	27.8	44.8	42.8	12.4	46.4	42.0	11.6	11.5	3.9	−15.4	13.1	3.1	−16.2	30.8	32.4
J	9	0.8	2.6	4.3	5.8	47.1	47.1	28.6	55.4	16.0	30.4	56.6	13.0	22.8	8.3	−31.1	24.6	9.5	−34.1	62.2	68.2
K	12	0.9	6.3	0.7	16.7	62.5	20.8	29.5	54.6	15.9	31.4	52.3	16.3	12.8	−7.9	−4.9	14.7	−10.2	−4.5	25.6	29.4
Sum of − divergences................														61.0	38.5	116.7	54.9	63.9	108.7		
Sum of + divergences................														71.2	111.3	33.7	79.6	94.5	53.6		
Sum of all divergences................																				432.4	455.0

§11. ADDITIONAL COMPARISONS IN EXPERIMENTS AA TO FF

I have made computations of the status to be expected in BB at trial 26 by schemes 25, 0, 0, and 25, −2, −2, and compared the results with the observed frequencies in trials 26 to 30 for the chicks that had the third set of ten trials. In this case the "pure individual" estimates of probable initial status were used and each chick was treated by itself as in Sections 7 and 9. 25, 0, 0 gives a fairly close fit. The minus, zero, and plus divergences are respectively 18, 1, and 22 for the rewarded 1 responses; and 21, 0, and 20 for the punished 5. For the punished 6 there are 9 minus, 4 at zero, and 28 plus. The total sum of divergences is 1800. By 25, −2, −2, it is 1888. 20, 0, 0, gives nearly as good a fit, the sum of divergences being 1832. 20, −2, −2, gives 2046.

I have done likewise in the case of CC and DD. For CC we have the following by scheme 20, 0, 0 (20 for the rewarded 6):

> Frequency of − divergences; for the punished 1 and 5, 18 (and 3 at zero).
> Frequency of − divergences; for the rewarded 6, 11 (and 1 at zero).
> Frequency of + divergences; for the punished 1 and 5, 27.
> Frequency of + divergences; for the rewarded 6, 12.
> Sum of − divergences; for the punished 1 and 5, 431; for the rewarded 6, 229.
> Sum of + divergences; for the punished 1 and 5, 329; for the rewarded 6, 331.
> Total sum of divergences: 1320.
> By 20, −2, −2, the sum of divergences is 1450.
> By 30, 0, 0, the sum of divergences is 1308. By 30, −2, −2, it is 1538.

For DD we have the following by scheme 5, 0, 0.[11]

> Frequency of − divergences: for the rewarded 1, 17 and 1 at zero; for the punished 5 and 6, 29 and 2 at zero.
> Frequency of + divergences: for the rewarded 1, 21; for the punished 5 and 6, 47.
> Sum of − divergences: for the rewarded 1, 567; for the punished 5 and 6, 826.
> Sum of + divergences: for the rewarded 1, 456; for the punished 5 and 6, 937.
> Total sum of divergences: 2786.
> By 5, −1, −1, the total sum is 2812.

I have also made various computations from the data of AA, EE, and FF in which the learning was slight or doubtful. In what learning there was, the punishment seems to have been as futile as in BB, CC, and DD.

Finally, we have Experiment G, reported in the next section.

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[11] The learning of DD suffered a set-back in trials 21 to 30.

§12. DATA FROM EXPERIMENT G

Experiment G was designed to provide very rapid learning and used only with chicks that had had the experience of Experiments AA to FF and were from 33 to 46 days old. In it, the three acts were differentiated not only by being in the locations 1, 3, and 6, but also by being themselves very different. The choice chamber was made very narrow, as shown in figure 2. At 1 the chick jumped upon a step 3 inches tall (S1) and then upon another step (S2) from which he could jump down into alley 1. At 3 the chick tried to squeeze through a rectangular opening 4 inches tall and $\frac{3}{4}$ inch wide made in a sheet of soft white tissue paper. At 6 the chick went down an alley. The exits from the alleys 1, 3, and 6 to the food pen were blocked by glass slides which were lifted when the chick jumped into 1, or pushed through

FIG. 2. Apparatus used in Experiment G. D is the entrance. S1 is a step 3 inches high. S2 is a step 3 inches higher than S1, or 6 inches in all. Pa is the tissue paper barrier as used in EE.

into 3, or entered 6, if the act in question was the one to be rewarded. For some chicks 1 was rewarded, 3 was punished by "thwarting", and 6 was punished by confinement. For other chicks, 1 was punished by confinement, 3 was rewarded, and 6 punished by confinement. For still other chicks, 1 was punished by confinement, 3 was punished by "thwarting," and 6 was rewarded. The confinement and thwarting were caused as in Experiments AA, BB, and EE.

Forty-three chicks had twenty trials in Experiment G, shortly after their experiences in AA to FF. For 14 of the chicks 1 (jump up to platform) was rewarded; for 15 of them 3 (push through paper) was rewarded; for the remaining 14, 6 (plain alley) was rewarded. The punishment for 1 and 6 was by con-

TABLE XVIII
Experiment GG. Records of all chicks

CHICK	REWARDED RESPONSE	TRIALS 1 TO 10										LENGTH OF INTERVAL	TRIALS 11 TO 20									
48	1	1	1	3	1	3	1	1	3	1	3	2 da.	1	1	1	1	1	1	1	1	1	1
59	1	3	3	1	1	3	3	3	3	1	3	"	3	1	3	3	3	1	3	1	1	3
60	1	1	1	1	1	1	1	3	1	1	3	"	1	1	1	1	1	1	1	1	1	1
61	1	1	6	1	6	1	1	1	3	3	1	"	1	1	1	1	1	1	1	1	1	1
65	1	3	1	3	3	1	3	1	1	1	1	1 da.	1	1	1	1	1	1	1	1	1	6
66	1	3	6	1	1	1	1	1	1	1	1	"	1	1	1	1	1	1	1	1	1	1
67	1	1	1	1	3	1	1	1	1	1	1	"	1	1	1	1	1	1	1	1	1	1
68	1	6	6	6	6	1	6	1	6	6	1	"	6	1	1	1	1	1	1	1	1	1
69	1	6	6	6	6	6	6	6	6	6	6	"	6	1	6	1	6	6	1	1	1	1
70	1	3	3	1	3	1	1	1	1	1	1	"	1	1	1	1	1	1	1	1	1	1
71	1	3	3	3	3	1	3	3	1	3	1	"	3	1	1	1	1	1	1	1	1	1
73	1	1	1	1	1	1	1	1	1	1	1	"	1	1	1	1	1	1	1	1	1	1
75	1	3	1	1	1	1	1	1	1	1	1	"	1	1	1	1	1	1	1	1	1	1
76	1	6	6	6	6	6	6	1	6	6	6	"	6	1	6	1	1	1	3	1	1	1
53	3	3	3	3	3	3	3	3	3	3	3	2 da.	3	3	3	3	6	3	3	3	3	3
55	3	3	3	3	3	3	3	3	3	3	3	"	6	3	3	3	3	3	3	3	3	3
56	3	3	1	3	3	6	1	1	3	3	3	"	3	3	3	3	3	3	3	3	1	6
57	3	3	3	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
58	3	1	1	6	1	1	6	6	1	1	6	"	3	6	6	6	1	6	1	6	6	6
93	3	6	3	1	3	3	3	3	3	3	3	1 da.	3	3	3	3	3	3	3	3	3	3
95	3	3	3	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
96	3	3	1	3	3	3	3	3	3	3	3	"	3	1	3	3	3	6	3	3	3	3
98	3	3	3	3	1	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
100	3	3	6	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
101	3	6	3	3	3	3	3	3	3	3	3	"	6	3	3	3	3	3	3	3	3	3
103	3	3	3	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
104	3	3	3	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
105	3	3	1	3	3	3	1	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
107	3	3	3	3	3	3	3	3	3	3	3	"	3	3	3	3	3	3	3	3	3	3
49	6	3	3	1	3	1	3	1	3	1	1	2 da.	1	1	6	1	6	6	3	6	6	6
50	6	1	1	3	3	1	1	1	1	1	1	"	1	1	1	6	6	6	6	3	6	6
51	6	3	1	1	1	1	3	1	3	1	3	"	6	6	1	1	6	6	6	6	6	6
52	6	6	1	1	1	1	1	1	1	1	1	"	1	1	1	1	1	1	6	6	6	6
77	6	3	1	3	1	1	6	6	6	6	6	1 da.	3	6	6	6	6	6	6	6	6	6
78	6	3	3	1	3	1	3	1	1	1	1	"	1	1	6	6	3	1	6	6	6	6
79	6	6	3	1	1	6	6	6	6	6	6	"	6	6	6	6	6	6	6	6	6	6
80	6	3	6	3	1	1	1	1	1	1	1	"	6	1	1	1	6	6	6	6	6	6
82	6	3	6	6	6	6	6	6	6	6	6	"	6	6	6	6	6	6	6	6	6	6
83	6	3	1	1	6	1	1	1	1	3	6	"	1	1	1	6	6	6	6	6	6	6
85	6	6	3	6	6	3	1	1	1	6	6	"	6	6	6	3	6	6	6	6	6	6
86	6	3	6	3	6	3	6	6	6	6	6	"	6	6	3	6	6	6	6	6	6	6
87	6	3	1	3	1	3	1	1	1	1	1	"	6	6	6	6	6	6	6	6	6	6
88	6	3	1	1	1	1	1	1	1	1	3	"	3	1	1	1	1	1	1	1	1	1

finement for 30 seconds; the punishment for 3 was simply the failure to escape, or thwarting.   The learning in Experiment G was rapid.   The facts appear in Table XVIII.

We have no means of estimating at all exactly the initial status of the chicks as individuals.   The frequencies for the group in the first trial were 7 for 1, 28 for 3, and 8 for 6, or 16, 65, and 19 per hundred.   Using $17\frac{1}{2}$ for 1 or 6 and 65 for 3 as approximate initial strengths for each chick, we have the following results when the rewarded C is 3:

12 c's in trial 1 followed by   8 c's in trial 2, 67 per cent, +2
 8 c's in trials 1 and 2 followed by   8 c's in trial 3, 100 per cent, +35
 8 c's in trials 1, 2 and 3 followed by 7 c's in trial 4, $87\frac{1}{2}$ per cent, +22.5

When the rewarded C is 1 or 6, we have the following results:

8 c's in trial 1, followed by   4 c's in trial 2, 50 per cent, +32.5
4 c's in trials 1 and 2 followed by   3 c's in trial 3, 75 per cent, +57.5
3 c's in trials 1, 2, and 3 followed by 2 c's in trial 4, 67 per cent, +49.5

Using the effect of the first three trials, as hitherto, the unweighted average per occurrence is +17 and the weighted average +15.

When the punished X is 3, we have the following:

16 x's in trial 1 followed by that same x 5 times in trial 2.... 31 per cent, −34
5 x's in trials 1 and 2 followed by that same x 1 time in trial 3..... 20 per cent, −45
1 x in trials 1, 2, and 3 followed by that same x 1 time in trial 4... 100 per cent, +35

When the punished X is 1 or 6, we have:

7 x's in trial 1 followed by that same x 5 times in trial 2.... 71 per cent, +53.5
5 x's in trials 1 and 2 followed by that same x 3 times in trial 3.   60 per cent, +42.5
3 x's in trials 1, 2, and 3 followed by that same x 3 times in
    trial 4... 100 per cent, +82.5

There is no doubt about the strengthening by reward and with no aid from punishment of the alternatives.   If we combine the discordant results as to punishment, weighting each according to the number of occurrences, we have a small average positive strengthening (+2) from a punished occurrence unaided by reward of the alternatives.

Comparing records in which trials 1 to 8 were rich in rewarded C's with records rich in punished X's, we have the following:

For the chicks for whom 1 or 6 was rewarded: There were 5 cases with 7 or 8 C's in the first 8 trials, including in all 36 C's and 4 X's (all 3's). In trials 9 and 10, there were 9 C's and 1 X. There were 4 cases with 7 or 8 of some one X, including in all 29 X's and 3 C's. In trials 9 and 10 there were 7 of the same X as had occurred in trials 1 to 8 and 1 of the other X. For the chicks for whom 3 was rewarded we have 11 cases of 7 or 8 C's in trials 1 to 8. These had C in trials 9 and 10 30 times out of 33. The only case poor in 3's was chick 58 with five 1's and 3 6's in trials 1 to 8, and with a 1 and a 6 in trials 9 and 10.

In applying method C (which uses various schemes of strengthening by reward and weakening by punishment) to the records from Experiment G, I shall modify the methods used hitherto because the learning was so rapid that a frequency of 100 per cent may well mean not a strength of just barely that or a little above it, or even somewhat below it, but a very substantial degree of overlearning. Our schemes of comparison are not appropriate to measure the influence of continued rewards on overlearning. The number of cases of 100 per cent success in BB, CC, and DD was not large, and their exclusion would only have strengthened the case for the failure of punishment to weaken the connection punished. But in G, we may get closer to the truth by setting up some rule to avoid such cases.

Our rule is arbitrary but impartial. It is (a) to measure final status by trials 9 to 13 unless the last four of them and seven out of the eight trials 13 to 20 are all correct. In that case, we go back to 8 to 12 or 7 to 11 or 6 to 10, until we have five trials, the last four of which include at least one wrong response. Also (b) we use no case where the number of occurrences in the training falls below 5. Applying this rule leaves us with the following data: Chicks 48, 59, 60, 61, 68, 69, 71, and 76 with 1 as the rewarded response; chicks 56, 58, 96, and 101 with 3 as the rewarded response; and chicks 49, 50, 51, 52, 77, 78, 80, 83, 85, 87, and 88 with 6 as the rewarded response.

We make computations with the expected and observed results in trials 9 to 13 for each of these. No fit is close.

The sum of the expected divergences from the actual ones is

2016 by scheme 20, 0, 0; 1990 by scheme 30, 0, 0, and a trifle more by scheme 50, 0, 0.   It is 2096 by 100, 0, 0.   By 30, −4, −4, it is 2332.   The facts stated concerning the cases rich in C's and those rich in X's and the original records make it certain that the application of any considerable decrease in strength per punished connection cannot improve the fit in any combination with any positive credit per rewarded connection from 20 to 50.

§13.   THE SIGNIFICANCE OF THE FAILURE TO RESPOND BY 1, 2 (OR 5),
OR 6 IN FIVE MINUTES

So far our treatment has been restricted to the choice from the 1, 2, and 6 (or 1, 5, and 6) responses provided the chick responded by some one of them.   We may now examine the records of failures to respond by any one of these, to see what, if anything, they contribute to knowledge of the effects of the rewards and punishments used.

As may be seen in Table I, many of the F's occurred in Trials 1 and 2.   These are not instructive to us, representing chicks that had weak tendencies to run into alleys, jump upon hurdles, or squeeze through or under doors at the start of the experiment in question.[12]

Many other F's are isolated instances, the chick returning to his previous behavior in the next trial.   These we will not study unless the study of the more pronounced cases indicates that something useful may be expected from them.

There remain the cases where the chick seems to have learned to diminish his runnings or jumpings or squeezings.   We take all cases of two or more successive F's occurring after 12 or more trials in the experiment in question.   We record their previous history in that experiment.[12]

In AA there were two such:

> 18, preceded by 2 rewarded and 11 punished, and
> 47, preceded by 9 rewarded and 8 punished

In BB there were none.

[12] We might study the F's at the beginning of EE in relation to the experiences in CC and DD, but it is hardly worth while, as there were only two of them.

In CC there were seven, as follows:

> 52, preceded by 1 rewarded, 14 punished, and 2 F's
> 66, preceded by 0 rewarded, 13 punished, and 2 F's
> 71, preceded by 2 rewarded and 10 punished
> 73, preceded by 0 rewarded and 20 punished
> 79, preceded by 1 rewarded and 19 punished
> 83, preceded by 0 rewarded, 10 punished, and 2 F's
> 100, preceded by 0 rewarded, 12 punished, and 2 F's

In DD there were four, as follows:

> 18, preceded by 1 rewarded, 12 punished, and 1 F
> 20, preceded by 0 rewarded and 14 punished
> 23, preceded by 0 rewarded, 10 punished, and 2 F's
> 85, preceded by 3 rewarded and 9 punished

In EE and FF there were none such.

Punishment is followed by F's oftener than reward is. But we cannot as yet tell how much of the difference is due to the strengthening of the general tendency to squeeze under or beside doors by the rewarding of the squeezing under 6 and beside 1, and how much is due to the weakening of it by punishment. The only practicable way to decide from our data is to set up schemes of assignments of values and observe the closeness of fit to the observed frequencies of squeezes of any sort and of failures to squeeze at all in 5 minutes. And the data seem too scanty to make this worth while. The contribution to the general argument would be very small compared to that from the data on 1, 2, and 6, and 1, 5, and 6, *inter se*. The data are available in Table I for anybody who may wish to make the computations.

§14. SUMMARY AND CONCLUSIONS

The results of all comparisons by all methods tell the same story. Rewarding a connection always strengthened it substantially; punishing it weakened it little or not at all.

Using Method A to compare the effect of a rewarded connection with that of a punished connection in trials 1 to 3, and using the weighted averages, the results were as shown in Table XIX, with an average strengthening for each rewarded connection amounting to 19.5 in BB, 28.5 in CC, and 24 in DD. The change due to one

punished connection averaged -1.2 in BB, $+4.3$ in CC, and -1 in DD, being on the whole a little on the plus side of zero.

TABLE XIX

Weighted average changes due to one rewarded occurrence and one punished occurrence in trials 1 to 3 (sometimes 11 to 13 also), according to the method used in estimating initial status)

		COM-BINED	PURE INDI-VIDUAL	AVER-AGE OF GROUP		COM-BINED	PURE INDI-VIDUAL	AVER-AGE OF GROUP
BB	Reward	$+23.5$	$+16$	$+18$	Punishment	$+0.5$	-3	-1
CC	Reward	$+25$	$+25.5$	$+35$	Punishment	$+5$	$+1$	$+7$
DD	Reward	$+25$	$+25$	$+22$	Punishment	0	$+1$	-4
G	Reward			$+15$				$+2$

TABLE XX

Summary of comparisons by Method C

EXPERI-MENT	METHOD OF ESTIMATING INITIAL STATUS	TRIALS THE EFFECTS OF WHICH ARE PROPHE-SIED	TRIALS USED TO TEST THE PROPHE-CIES	A SCHEME OF ADDITIONS FOR C, X_1 AND X_2, WITH CLOSE FIT	SUM OF DIVER-GENCES OF PROPH-ECY FROM ACTUAL, BY A	B SCHEME OF ADDITIONS WITH WEAKENING FOR PUNISHMENT	SUM OF DIVER-GENCES OF PROPH-ECY FROM ACTUAL, BY B
BB	Combined	1 to 15	16 to 20*	$+25, 0, 0$	3,680	$+25, -2, -2$	3,690
CC	"	"	"	$+40, 0, 0$	2,208	$+40, -2, -2$	2,280
DD	"	"	"	$+15, 0, 0$	3,420	$+15, -1, -1$	3,452
BB	Av. for group	1 to 8 / 11 to 18	9 and 10 / 19 and 20	$+30, 0, 0$	212.6	$+30, -2, -2$	226.8
CC	"	1 to 8 / 11 to 18	9 and 10 / 19 and 20	$+60, +1, +1$ / $+60, 0, 0$	245.0 / 250.4	$+60, -4, -4$	305.0
DD	"	1 to 8 / 11 to 18	9 and 10 / 19 and 20	$+15, 0, 0$	432.4	$+15, -1, -1$	455
GG	"	1 to 8	9 to 13	$+30, 0, 0$	1,990	$+30, -4, -4$	2,332
BB	Pure indiv.	1 to 25	26 to 30	$+25, 0, 0$	1,800	$+25, -2, -2$	1,888
CC	"	"	"	$+20, 0, 0$ / $+20, 0, 0$	1,832 / 1,320	$+20, -2, -2$ / $+20, -2, -2$	2,046 / 1,450
DD	"	"	"	$+30, 0, 0$ / $+5, 0, 0$	1,308 / 2,770	$+30, -2, -2$ / $+5, -1, -1$	1,538 / 2,812

*Checked by the use of 16 and 17.

Using Method C the results were as shown in Table XX. In every one of the ten comparisons there is evidence that punished occurrences of a connection weaken it very slightly if at all. Except in BB (1 to 15 on 16 to 20) where 25, $+1$, $+1$ may be as good as, or a bit better than, 25, 0, 0, the probabilities are that

some scheme of $+$, 0, 0 can be found better than any scheme of $+$, $-$, $-$.

I am confident that any reasonable systems of treatment of the records of these animals will give the same result as the simple additive system which I have used. The data are available in Tables I and III for anybody to use with any system of his choice.

If we take the results at their face-value, the punishment by confinement in BB seems to have been a little more efficacious than the punishment by thwarting in CC and DD, the punished occurrences in CC and DD acting actually to strengthen the connections. The difference may, however, be due to the occasional cases when the chicks ran back out from the BB alleys 5 and 6, before the experimenter could put in the slide to confine them. There was naturally in such cases a certain increase of aversion toward the 5, 6 alleys and the 5, 6 end of the choice chamber.

It is, at first thought, somewhat surprising that in CC and DD the connections leading to failure and so to turning away from, or disregard of, the door in question should be actually slightly strengthened by the total experience. As one watches the animals and sees them, after one or more pushes at the fixed door, turn to some other act, one tends to think that the failure has caused the turning away or disregard and that this in the future will cause increased turning away or disregard. But this is probably the same fallacy as that of inferring in general from the decline in X's and increase in C's that the X's weaken themselves. The failure does *not* cause the animal to step and turn away. The real cause of his stopping is that in the ups and downs of the variation of the competing tendencies (to push here, to push there, to peep, to walk about, to jump, etc.) the animal comes to a status where "push at 2" is low and some other tendency is high. The chick stops the punished response temporarily for the same general reason that he began it—the comparative strength of his different tendencies at the moment. The thing most worth observing in the behavior immediately following a punishment by thwarting is the great frequency of a repetition then and there of the punished response. One reason why the retreats or turn-

ings away or neglects do not much influence permanent learning
is that the animal so often does *not* retreat, but at once repeats
the punished act.

On the whole the results of these experiments offer strong sup-
port to the conclusions quoted on page 1f, to which we were
led by our experiments with human subjects.   They are almost a
crucial test.

APPENDIX I.   THE DETERMINATION OF INITIAL STRENGTH OF 1, 2, 5 AND 6 FOR EACH CHICK

In the case of chicks 48 to 107, the data used to estimate the
relative strength of 1, 2 and 6 in experiments using these alleys
are derived from Table I and Table XXI.   The 6 preliminary
trials with AA, DD, and FF are used as follows: Any F's or
omitted trials are prorated amongst 1, 2, and 6 according to the
frequencies of these for the chick in question.   The frequencies
of 1, 2 and 6 are then multiplied by 10.   The three first learning
trials are treated in the same manner.

The occurrences of 1 and 6 in the first 20 trials of AA are mul-
tiplied or divided by whatever is necessary to make their sum
20.[13]   The occurrences of 1 and 2 in the first 20 trials of FF are
treated in the same way.   So also the occurrences of 2 and 6 in
the first 20 trials of DD.

We have then 90 points of frequency credit from the prelimi-
nary and first trials, and 60 points of frequency credit from the
punished occurrences of 1 and 6 in AA, 1 and 2 in FF, and 2 and 6
in DD, equal weight being attached to each of the three experi-
ments.

We sum the credits for 1, 2 and 6 respectively and divide by
150.   The results, shown as column I of Table III and referred
to as *pure individual estimates for any 1, 2, 6 experiment*, are justi-
fiable as estimates.

It is, however, probable that estimates nearer the truth can be
obtained by giving some weight also (1) to the corresponding
facts for each chick's response by 1, 5 and 6 in BB, CC, and EE,

[13] In the few cases in which neither 1 nor 6 occurred at all in the first 20 trials in
AA, the strength is assumed to have been equal and a score of 10, 10 is used.

TABLE XXI

Response of chicks 48–107 in twelve preliminary trials in each of which all three responses were rewarded

CHICK	RESPONSES IN FIRST TRIALS OF						RESPONSES IN SECOND TRIALS OF					
	AA	BB	CC	DD	EE	FF	AA	BB	CC	DD	EE	FF
48	6	5	5	6	5	6	6	6	5	6	5	6
49	F	5	5	1	5	2	2	5	5	2	5	2
50	2	1	5	2	5	2	2	5	5	2	5	2
51	6	5	1	6	5	6	2	5	5	2	5	6
52	2	1	5	2	5	6	6	6	5	2	5	6
53	2	5	F	2	5	6	6	6	F	1	5	6
55	F	1	5	2	6	2	2	5	5	2	5	2
56	F	5	F	2	5	6	6	5	5	2	6	6
57	2	1	1	2	5	2	6	5	1	2	5	2
58	2	1	5	2	5	6	2	5	5	2	5	6
59	2	5	5	2	5	6	6	5	5	2	5	6
60	2	5	5	1	5	6	6	5	6	2	5	6
61	2	5	5	2	5	6	6	5	5	2	5	6
65	5	5	6	2	5	6	6	5	F	2	5	6
66	F	F	F	1	5	F	F	F	F	2	5	2
67	F	5	F	2	5	2	F	5	F	F	5	2
68	F	1	F	2	5	1	F	5	F	6	5	2
69	F	F	F	1	5	2	F	5	F	2	5	6
70	2	5	5	2	5	2	2	5	5	2	5	6
71	2	5	5	2	5	2	2	5	F	2	5	6
73	F	F	5	2	5	2	6	1	5	2	5	2
75	2	5	5	1	5	2	6	5	1	2	5	6
76	F	F	F	2	1	2	F	1	F	F	1	6
77	6	6	5	2	5	2	6	5	5	2	5	6
78	F	1	F	2	5	2	2	5	F	2	6	2
79	1	1	5	2	5	2	2	5	5	2	5	2
80	2	5	F	2	5	6	2	5	F	2	5	2
82	6	5	F	6	5	2	F	1	F	2	5	2
83	2	5	F	2	5	2	2	1	F	2	5	6
85	2	1	5	2	5	2	2	5	5	2	5	2
86	5	5	5	1	1	2	6	5	F	2	6	6
87	2	F	5	2	5	1	2	5	5	2	5	6
88	2	F	1	1	5	2	2	1	5	2	5	2
93	6	5	5	2	5	2	2	5	5	2	5	6
95	6	5	5	6	5	2	F	1	F	2	5	2
96	2	1	F	2	5	2	6	1	5	2	5	2
98	2	1	5	2	5	2	2	5	F	2	5	6
100	2	5	F	2	1	2	2	1	F	2	5	2
101	2	F	F	2	5	1	2	5	F	2	5	6
103	2	5	5	6	5	2	6	5	6	6	5	2
104	F	F	F	2	1	2	F	1	F	2	5	2
105	6	F	5	6	5	2	2	5	5	2	5	6
107	6	1	5	6	5	2	2	5	5	2	5	6

and (2) to the average for all chicks in DD, which is the particular experiment where we need to know the initial strengths of the 1, 2 and 6 tendencies.

Consequently we have made a second estimate, called the *special combined DD estimate*, in which the pure individual estimate for a chick for any 1, 2, 6 experiment has a weight of 3, the pure individual estimate for that chick for any 1, 5, 6 experiment has a weight of $1\frac{1}{2}$, and the initial frequency in DD first trial for all chicks has a weight of 1. This is shown as column II of Table III.

The pure individual estimates for any 1, 5, 6 experiment are computed in just the same manner as the 1, 2, 6 estimates, except that the six preliminary trials and the three first learning trials are those with BB, CC and EE, and that the unrewarded occurrences used are of 1 and 6 in the first 20 trials of EE, 1 and 5 in the first 20 trials of CC, and 5 and 6 in the first 20 trials of BB. The cases where neither 1 nor 6 occurred in the first 20 trials in EE are treated as they were in AA. There are many of them.

There was one case in CC where neither 1 nor 5 occurred in the first 20 trials. Since this chick had no 1's in all its initial and first-trial responses in all experiments, and only a single 1 in all its unrewarded responses other than in CC, I assumed that if it had shown any 1's or 5's, the former would have been relatively very few, and have used 2 and 18 as the entry.

We compute also for 1, 5, 6 a *special combined BB estimate*, and a *special combined CC estimate*, corresponding to the special combined DD estimate for 1, 2, 6. These and the pure individual estimates all appear in Table III, columns III, IV, and V, respectively.

Chicks 1 to 47 did not have preliminary trials with Apparatus AA, BB, CC, etc. but instead had 2 preliminary trials and 8 training trials with Apparatus A, B, C, D, E, and F, all of which offered a choice from all six alleys, all having glass slides at their food-pen ends. Except for these facts and for the third fact that A had plain openings $3\frac{1}{2}$ inches wide and 12 inches high instead of the hurdles of AA, the acts involved were the same as in AA, BB, CC, etc. We use the frequency in the last 4 or 5 of these

training trials to obtain a measure of the chick's tendency to avoid the end positions.[14]

Our main reliance, however, has to be upon the first learning trial with AA, BB, etc. and upon the unrewarded responses in the first twenty trials. Our system attaches twice as much weight to each of these two sources as to the frequencies in A, B, C, etc. To obtain the 'pure individual' estimate for DD, we multiply each occurrence of 1, 2 or 6 in the first trials in AA, DD and FF, by 20. We treat the unrewarded responses in the first 20 trials of AA, DD and FF just as in the case of chicks 48 to 107. We count the 1's, 6's, and 2's, 3's, 4's and 5's lumped together, in the last 5 trials of A, B, C, D, E and F, and use the sum of the 2's, 3's, 4's, and 5's as an estimate of what 2 would have been if the choice had been restricted to 1, 2 and 6. We then sum for 1, for 2, and for 6, and divide the sums by 150. The results appear in column I of Table III.

To obtain the 'pure individual' estimate for 1, 5 and 6 in BB and CC, we do the same, except that BB, CC, and EE replace AA, DD, and FF, and that the sum of the 2's, 3's, 4's and 5's is now used to infer 5.

The combined estimates are obtained in the same way that was used for chicks 48 to 107.

APPENDIX II.   FREQUENCY AND LEARNING

Although the doctrine that ordinary animal learning is due to the greater frequency of one connection has been disproved again and again since 1911, it crops up repeatedly. So it may be worth while to show its futility in the case of the present series of experiments.

The chicks choose 2 or 5 more often than 1 or 6. So if we take all the cases where 1 or 6 is rewarded while 2 or 5 is punished, we shall have material where little frequency with much reward competes with much frequency with no reward. We simply count up occurrences for the first six trials and the next four trials,

---

[14] We could use all eight, since there is no demonstrable learning during the eight.

and observe whether the response that was most frequent in the first six gained in frequency in the next four.   It does not.

Using BB, CC, and DD, and multiplying the number of occurrences in trials 7 to 10 by 1.5, we find the following:

> If the most frequent response in 1 to 6 occurred 6 times, it drops to an average of 4.3
> If it occurred 5 times, it drops to an average of 3.7.
> If it occurred 4 times, it drops to an average of 3.2.
> If it occurred 3 times, it drops to an average of 2.4.

The use of trials 11–16 and 17 to 20, or of any other division showing the effects of learning, shows a similar failure of the more frequent to gain in strength thereby.

COMPARATIVE PSYCHOLOGY MONOGRAPHS

THE RELIABILITY OF THE MAZE AND METHODS OF ITS DETERMINATION

BY

KENNETH W. SPENCE

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THE RELIABILITY OF THE MAZE AND METHODS OF ITS DETERMINATION[1]

KENNETH W. SPENCE[2]

From the Psychological Laboratory of McGill University

CONTENTS

1. INTRODUCTION

A perusal of the recent psychological journals, especially those concerned with experimental problems, will reveal the ever increasing popularity of the maze or labyrinth method in psychological research. Warden and Warner (31), in a review of the literature of animal maze studies up to the year 1927, cited 138 references, and during the same period about 50 studies were

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reported in which this instrument had been used for experimentation with human beings. The last few years have witnessed the development and use of a still greater number and variety of patterns and types of mazes. Nearly every new experimental journal contains a description of some novel maze device.

While the development of new types of mazes should certainly be given every encouragement, there is also, from the point of view of developing a testing device for measuring learning ability, a considerable need of careful study of the various existing types with a view to standardizing a few of the most promising ones for more universal use. One of the most important problems involved in such a study is that of reliability and it is with this problem that the investigation reported below is concerned.

Two aspects of this problem of the reliability of the maze as a measuring instrument are considered; first, the significance of the various methods of determining the reliability of maze scores, and secondly, the degree and significance of the reliability of the maze used in the present experiment. While the experiment was concerned only with human subjects the discussion of the methods of determining reliability considers the problem in connection with animal studies also. In fact most of the investigations dealing with this problem have been animal studies.

2. EXPERIMENTAL PROCEDURE AND DATA

A. Apparatus

The two mazes used in this study were made in the laboratory workshop by the writer. They were both high relief finger mazes constructed, as suggested by Miles (20), of No. 14 copper wire ($\frac{1}{16}$-inch in diameter) bent into long square cornered staples of suitable lengths for the pattern desired and fastened on to a wooden panel. The panel was 18 inches square, 12 inches of which was covered by the pattern. Ordinary rubber feet were fastened at the corners in order to make the panel more satisfactory while in use.

Two patterns were used, both being of the multiple T type. They are shown in figures 1 and 2. In this multiple T type of pattern every true path runs into a cross path leading either to

the right or the left. In one direction is a blind alley, in the other the true path. The subject must learn to choose the cor-
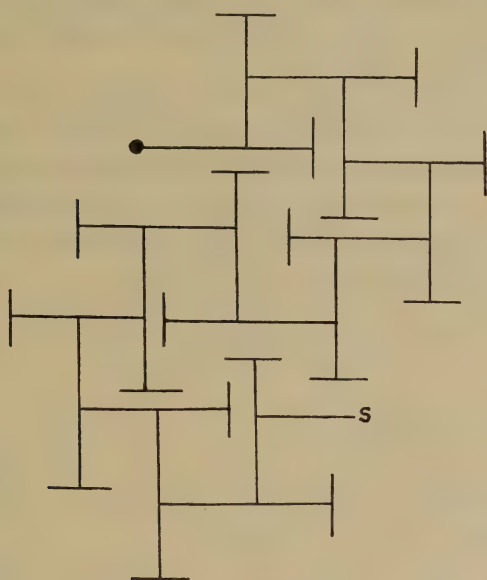
FIG. 1. MAZE I
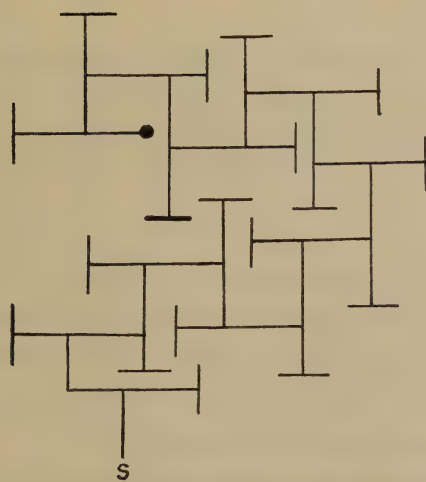
FIG. 2. MAZE II

rect alternative at each successive junction. This calls for a
somewhat different type of adjustment than in the kind of pattern
in which the true path runs off at right angles from the preceding
segment of the true path, the latter continuing straight ahead to
form a blind alley. (See Figure 3.)

This latter type of pattern calls for timing of movements so as
not to run by the true path leading off at right-angles; which is
quite likely to happen even with narrow wire pathways, especially
if the subject happens to be pressing on the opposite side of the
wire. There is no possibility of this happening in the multiple T
pattern, it will be noticed. (See Figure 4.)

The blindfold equipment consisted of a pair of automobile
goggles, the glasses of which were covered with adhesive tape.
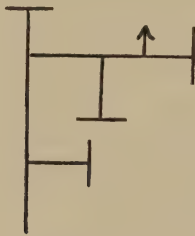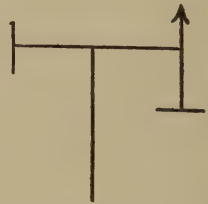
FIG. 3   FIG. 4

This blindfold permitted the subject to keep his eyes open if he
so desired and thus to be quite comfortable even for long sessions
of work. It is interesting to note that several of the subjects
preferred to keep their eyes closed even behind the blindfold.

B. Subjects

There are two different groups of subjects. The main group
consisted of students of the investigator's elementary psychology
laboratory classes, which are largely made up of sophomores.
This group will be designated as Group I. Only students who
volunteered were used as subjects in the hope of obtaining as
highly a motivated group as possible.

Table I shows the means, the standard deviations and ranges
of the Otis Intelligence test scores (average of Forms A and B,

Higher Examination) of this group and of the elementary psychology class from which it was taken.

It will be seen that the average scores are practically the same. The experimental group, however, has less of a spread than the class. The extreme narrowness of the ranges of intelligence in these groups becomes apparent when we compare their standard deviations with the standard deviation of 13.8 obtained for a group of pupils in grades 7 to 12 (calculated from the P. E. of score which is given in Otis' manual of directions).

Data on Maze I are also available for a group of Y. M. C. A. boys from age 11 to 15 and will be used in connection with some

TABLE I

Otis Self-Administering Test

	NO.	MEAN	S.D.	RANGE
Group I (Main).........................	50	59.1	6.4	44–71
Elem. Psy. Class......................	160	59.0	8.6	40–73

TABLE II

Y.M.C.A. BOYS (N 25)	MEAN	S.D.	RANGE
Intermediate Score......................	46.56	11.4	29–66
Higher Score...........................	31.2	11.0	16–53
Otis I.Q................................	101.0	10.6	83–124

of the discussions. One of the advanced experimental psychology class students, Mr. Webster, carried out the actual testing of these boys under the writer's direction. None of the subjects, either of the college group or of the Y. M. C. A. group, had ever had any previous experience with mazes. The intelligence test scores of the Y. M. C. A. group are given in Table II above. The Otis Intermediate Examination was used with this group. The equivalent Higher Examination scores and Otis I.Q. scores (based on Otis' interpretation chart) are also given.

C. Method of Procedure

The subject was seated at a desk and after the blindfold had been carefully adjusted was given the following verbal directions:

"In this experiment you are going to learn the form of a raised pattern by passing your finger over it while your eyes are blindfolded.   The pattern is made of wire fastened into a wooden panel making different pathways.   All the paths which make up the pattern are just of this width and feel like this."   [E then demonstrated by placing S's forefinger on the sample.]

"Now each turn in the pattern, or maze as it is called, is right-angled, moreover, the path never continues straight ahead.   Between the starting and the ending places there are many blinds or culs-de-sac, places where you will run off the track because the wire ends without connecting up with any other wire.   Each time you go into a blind an error will be counted.   If at any time you should start to retrace I will stop you and start you in the right direction again.   An error will be counted, however."

"In each trial, then, you are to follow along the wire path from the starting place, exploring the different paths in an attempt to reach the ending place with as few errors as possible.   The object is to learn to run through the paths without making any errors, i.e. without going into any blinds.   This will be considered accomplished when you have made three perfect runs in succession or four perfect runs out of five."

"The time of each trial is taken by means of a stop-watch.   As a final suggestion remember that accuracy is more important than speed."

The hand not used in tracing was then placed in a resting position at its corner, and the index finger of the other hand at the entrance of the maze pattern ready to start at the signal "Go." As many trials were then given as was necessary to satisfy the criterion.   Those subjects who learned the maze in less than ten trials were required to complete the ten so that there would be complete time records of at least ten trials for every subject. The extra trials were not included in these subjects' scores however.   The subjects who took a considerable number of trials were asked if they were tired and offered the opportunity of a short rest.   Not one, however, admitted any fatigue or availed himself of the opportunity for a rest.

The second maze was given about three weeks after the first one, the time often varying from this by three or four days for some subjects.   It is not likely, however, that these differences in time influenced the results in any manner as they were relatively

slight. Also the subjects were told immediately after they fin-
ished the first maze that they were going to have to learn a second
one some time later which would be different, and that they should
forget about the first one. They were warned again just before
they started to learn Maze II that it was a different pattern than
Maze I though of the same type. The latter part of the direc-
tions were also repeated.

D. Method of Recording and Data

The performances of each individual were carefully noted upon
prepared data sheets, which permitted keeping accurate record

TABLE III

Maze I Results

GROUP	TRIALS		SINGLE ERRORS		DOUBLE ERRORS		TIME	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
I.	22.6	10.6	57.8	29.4	80.8	37.7	802.9	377.5
M.	22.3	10.3	57.2	30.8	81.9	42.6	821.4	424.7
F.	22.9	10.9	58.5	27.9	79.7	31.7	782.9	318.2
II.	28.2	16.1	82.0	48.4	124.4	78.3	1078.9	614.7

TABLE IV

Maze II Results

GROUP	TRIALS		SINGLE ERRORS		DOUBLE ERRORS		TIME	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
I.	18.3	9.0	39.5	22.2	54.1	28.0	589.2	297.3
M.	18.7	10.6	39.9	25.8	54.0	29.8	624.5	354.5
F.	17.9	7.9	39.1	17.4	54.1	25.1	550.9	221.0

of where all the errors were made. The data taken included the
number of trials required to complete the learning (including
three criterion trials), the number of errors on each trial, the
number of attempted retraces and the time of each trial.

The possible errors were of two types: (1) any entrance into
a blind alley and (2) any attempt to retrace toward the beginning.
These latter, which were not recorded against the blinds, but on

a separate column on the data sheet, were relatively insignificant in number.
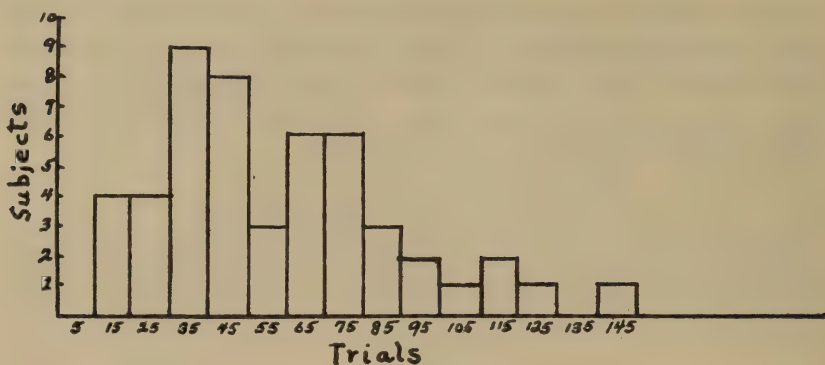
FIG. 5. GROUP I—MAZE I
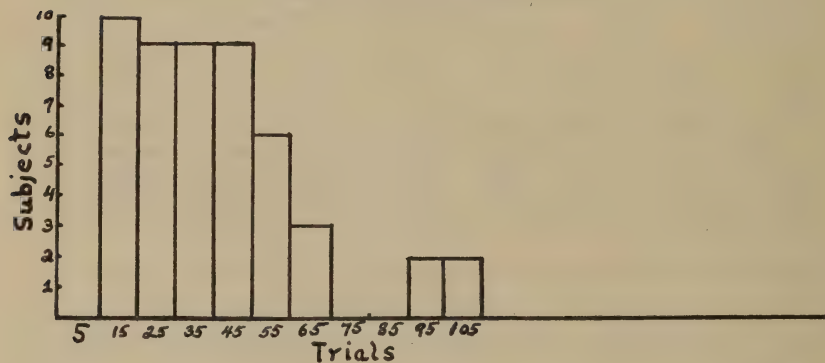
FIG. 6. GROUP I—MAZE II
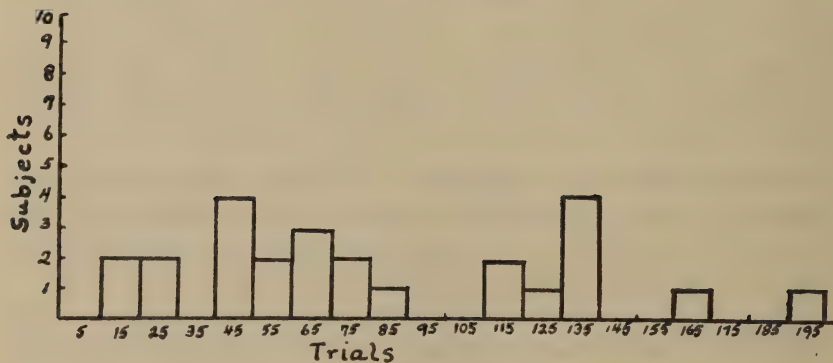
FIG. 7. GROUP II—MAZE I

In such maze patterns with cross pieces at the blinds there are two possible ways of scoring Type I errors: (1) counting one error, whether the subject goes off both ends of the cross piece or just one end of it, (2) counting an error each time the subject goes off the end of a path. In this latter method two errors would be counted for going off both ends of the cross piece.

Tables III and IV show the results for the two mazes. As in Table I, Group I is the college group. This main group has further been divided according to sexes into two sub-groups, M and F for purposes of comparison. Group II consists of the Y. M. C. A. boys. Graphs showing the nature of the distribution of the scores (single errors) are given in figures 5, 6 and 7.

3. THE CONCEPT OF RELIABILITY

Before considering the past literature concerned with the problems of the reliability of the maze, the writer would like first to review the concept of reliability and the methods of measuring it as originally developed in the field of mental testing. Such concepts as "reliability" and "reliability coefficient," unfortunately, have not always been properly interpreted or correctly employed when carried over to maze data. This preliminary discussion is most brief; a further discussion of this concept will be given in connection with the examination of the methods that have been used in measuring the reliability of the maze and in the discussion of the results of the present study.

By the reliability of any measuring instrument, an intelligence test for example, is meant the amount of agreement between the results secured from two or more applications of the test to the same subjects. The reliability of an instrument is determined, then, by the consistency with which it differentiates the various individuals in a group on different occasions. Thus, if we measure a group of subjects with two similar or duplicate tests and find that the order of merit is identical on the two forms, we should say that the test, either form, gives a perfectly reliable measure of whatever ability it purports to measure. If, however, there are numerous changes in the order of the subjects from one time to the other the test is obviously inconsistent and unreliable.

The reliability of a test can be measured most conveniently by correlating the scores obtained on the two different occasions. This correlation coefficient is called a "reliability coefficient," a term introduced by Spearman (23) in 1904. It is important to note that the reliability coefficient as Spearman defines it, and uses it, in his formula for the correction of the correlation between two variables for attenuation is that correlation between two comparable tests, and not between scores obtained by repeating the same test twice. This latter involves a correlation of the errors of measurement. We will leave this point, however, to a later discussion.

In the same article Spearman gives the following simple example which admirably illustrates the notion of a reliable as opposed to an unreliable instrument: "Suppose three balls to be rolled along a well-kept lawn: then the various distances they will go will be almost perfectly correlated to the various forces with which they are impelled. But let these balls be cast with the same inequalities of force down a rough mountain side; then the respective distances eventually attained will have but faint correspondence to the respective original momenta" (23, p. 89).

That is, our measuring device is either like the mountain side, where chance plays the determining part or like a well-kept lawn, a carefully groomed golf green for instance, where individual ability and not chance is the chief factor.

One further point should be brought out here and that is the fact of variability within the individual of the function being measured. That is, no measurement from a psychological instrument may be said to be a perfectly accurate one, i.e. the true measure; for even supposing we had a perfect testing instrument the measure we would obtain from it on different occasions would still vary slightly because of this variability within the individual. Such minute fluctuations are the result of a multitudinous number of small forces which cannot in practise be entirely controlled.

4. REVIEW OF PAST STUDIES

It was not until the year 1922 that any systematic investigation of the problem of the reliability of the maze was made. There

were, however, two or three incidental references to the problem in the earlier experimental work. In 1917 Webb (32), in a study of transfer of training and retroaction, reported correlations that bear upon the reliability of the maze. He had a group of subjects learn a maze (A), then learn another maze, and then relearn Maze A thirty days after it had been learned the first time. Another group learned Maze A and then thirty days later relearned it. The correlations (rank method) calculated from only eleven subjects at most in his rat groups, and six subjects in his human groups, are on the whole very small. Only sixteen of the thirty, in fact, were positive. Heron (9), writing later, referred to these correlations of Webb and claimed that their lowness was evidence of the low reliability of the maze Webb used. Such a conclusion does not necessarily follow however, for the measures correlated are not measures of the same capacity. One is a measure of the efficiency of the maze-learning capacity and the other is a measure of a quite different kind of process, namely the ability to relearn or re-establish a previously acquired habit. The correlations cited by Webb may be more accurately described as measures of the relationship between these two different processes, attenuated of course by the chance errors in the measures.

Bagg (2), in a study of individual differences in mice, also reported correlations that have a bearing upon the problem we are discussing. He first gave a group of mice seventeen consecutive trials on a maze that required the animal to learn which of two doors to push open between each section. He then gave them what he termed an "interference test" in which twelve trials were given in the same maze as before except that the doors that had been open in the first experiment were now closed and vice versa. Later ten more trials were given with the doors arranged as in the "interference test." He found a correlation of 0.55 between the sums of the time for trials 3–17 of the first test and the first two trials of the "interference test," and one of 0.49 between test one and the time on trials 2–10 in the "interference test." He also reported a correlation of 0.46 between the time scores of trials 3–7 and trials 13–17 of the first test. This

latter type of correlation, i.e. between two segments of the learning curve, has been used extensively as a measure of reliability. We leave the discussion of it, as well as the others, to the next section.

In the year 1922 Hunter (13) reported an experiment on habit interference in the white rat and human subjects in which he gives the correlations between "the ability displayed in mastering one habit and the ability displayed in breaking that habit by forming an antagonistic one." That is, these correlations are between two mazes one of which is just the reverse of the other. He found that the correlations were much higher in the case of the human subjects. For instance the Pearson correlation for the total trials required in learning the two habits was 0.31 $\pm$ 0.10. For total time the foot-rule correlation was 0.126 $\pm$ 0.07. With thirty one human subjects he obtained a foot-rule correlation between total time in the two habits of 0.64 $\pm$ 0.07, while the coefficient for total trials was 0.68 $\pm$ 0.07. These foot-rule values give Pearson r's of above 0.80. In considering the extremely small size of the correlations with the rats Hunter suggested the possibility that chance factors play such a predominant part in the maze that it is impossible to obtain consistent results.

The first to report an investigation concerned primarily with the problem of reliability in this field were Heron and Hunter (8, 14). The experimental work was carried out under the direction of Hunter at the University of Kansas and was coincident with his study of habit interference, some of the data of the latter in fact being used in the study of reliability. The report is in two parts, the first part by Heron being concerned with the reliability of the inclined plane problem box; the second part, by Hunter (14), was a correlation study of maze learning. Heron's study, while not concerned with the maze, is important from the point of view of the methods used in determining reliability. He attacked the problem with three different methods. In the first method he correlated the sums of the time on the odd and even trials, the assumption being that "if chance errors are not operating to a serious degree to vitiate the practice effects, there should be a high correlation between the odd and

even trials." The second method used consisted in correlating the results in six trials with the performance in six more trials sixty days later. The assumption here was that if the apparatus is reliable the animal should rank the same on the two testings, which are as far as possible under identical conditions. The method is not quite the same as correlating complete learning with relearning as Webb did. It is somewhat similar to Bagg's correlation of segments of the learning series (i.e. the correlation of trials 3–7 with trials 13–17), differing from it in the length of the interval between the trials correlated. Neither of the correlated values, however, are measures of learning ability, as is shown by the fact that the first six trials in Heron's experiment correlated only to the extent of 0.51 with complete learning. The factor of retention also comes in to increase further the ambiguity of this coefficient. Heron, himself, recognized this latter difficulty and stated that "all attempts to calculate the reliability of any situation in which the rat is used will undoubtedly encounter this difficulty of retention" (8, p. 28).

The third method used by Heron in determining the reliability of his problem box apparatus consisted in correlating the performance (time) in six trials in the problem box with the performance in six trials in a maze sixty days later. Such a correlation coefficient, however, proves nothing concerning the reliability of either of the two pieces of apparatus; it is essentially the procedure of correlating a test with a criterion. Such a coefficient might be a measure of the validity of the problem but not of its reliability. All three of the methods were consistent in giving very small correlations, which lead Heron to conclude that the problem box was not a very reliable measuring instrument.

In the second part of the report, on maze learning, Hunter considered several problems, among them the rôle of chance in maze learning and the influence of the degree of complexity of the maze upon this problem. In connection with this latter problem Hunter found that the intercorrelations between the various tenths of the learning curves were lower in the case of a complex maze than in a simple maze, and he considered this evidence that the simpler maze offered more dependable data.

This, he argued, was what one should expect a priori, as the simpler the maze the less opportunity there should be for chance to play a part.

In the year 1924 no less than four different studies of the problem of the reliability of the maze were reported. Three of these were concerned primarily with this problem. We shall begin by considering Heron's work (9), which was the most extensive. The primary purpose of this investigation was to determine the extent to which the scores of individuals in learning the stylus maze are due to individual ability, and to what degree they are the result of unknown and uncontrolled factors, i.e., chance. Heron's method in this study was that of inter-correlating the scores made in learning several different mazes. The supposition was that if these correlations were high it was evidence that some constant factor, he called it "the individual's ability to learn mazes," was determining the scores of each individual on the different mazes. If, on the other hand, the correlations were low, it would be apparent that a score on any maze was largely a matter of chance and therefore not indicative of individual ability. This correlating of the scores on different mazes is essentially the method used in calculating "reliability coefficients" when two different forms of an intelligence test are available, providing the different mazes can be considered comparable. We will later consider the conditions that must be obtained before the correlation between two tests can be considered a valid reliability coefficient.

Heron found that the correlation between the five mazes were on the whole small, averaging 0.32 and ranging from 0.02 to 0.65. "The net result of these correlations," he states, "is to show that the relative influence of chance and individual ability varies with the pattern of the maze. In some mazes chance is so effective that the influence of individual ability upon the score is practically negligible. In other mazes, roughly speaking, chance and individual ability play about equal parts in the production of the scores. In no maze can it be said that individual ability consistently plays a dominant part" (9, p. 14). He further concluded that mazes which allow of a generalization, i.e. some plan which,

when once known, enables the subject to run the maze without further difficulty, are the more unreliable. This, he claims, is due to the fact that among a group of individuals at about the same level of intelligence the time required to make a generalization is largely a matter of chance.

Hunter and Randolph (15) reported in this same year the results of further investigations of this problem of the reliability of the maze carried out at the Kansas laboratory. The report includes fourteen experiments in all, human subjects being used in three and rat subjects in eleven. The procedure used was similar to that employed by Heron in his study of the reliability of the problem box. It consisted in giving the subjects a series of trials in the maze (six with the human) and then after a considerable interval of time another series of trials. The results of the two series were then correlated. The four correlations obtained from the human subjects were 0.49, 0.03, 0.06 and 0.51. The last three correlations were of the first and second series, first and third series, and second and third series of trials. Using the same procedure with nonsense material and the method of paired associates Hunter found a correlation of 0.58 between the test and the retest.

In concluding the report Hunter summed up his views "concerning the present status (1924) of the maze as a method of studying the learning process," in part, as follows: "No maze, either for rats or humans, is analyzed sufficiently to justify the unqualified acceptance of the data gathered from it with reference to the determination of individual and group differences in learning. Habit-formation does take place in the maze, and most subjects can be trained to the point of mastery, but a comparison of subjects upon the basis of the data gathered on the learning process itself is of very uncertain value. At the present time the T-shaped maze seems the most dependable for rats. With humans, Heron's Maze 4 and Hunter's simple A maze are suggested for use" (15, p. 440).

The remaining two studies of the reliability of the maze published in 1924 were from the University of California psychological laboratory. One is really an appendix to the other, which

is concerned primarily with the problem of the inheritance of maze learning ability in rats. In this major study (27) an attempt was made to ascertain if possible the degree of the reliability of the maze scores. Several different methods were used. First the maze scores for different runs were correlated against each other. The resulting coefficients were even lower than those reported by Hunter. The next method used was that of correlating the combined scores for a group of early runs against a group of late runs and also the method employed by Heron in his study of the problem box, namely, the correlation of combined scores on the odd trials with those on the even trials. These correlations were also low. Three other schemes were also tried to see if more consistent results, i.e. higher reliability coefficients, could be obtained. The first of these consisted in weighting the scores in individual runs in terms of their respective standard deviations. This method allows the score in each trial to have an equivalent effect upon the combined score. The procedure in the second method was to discard the record of any rat which in some one individual trial tended to vary excessively from the mean of that trial. The third method consisted in scoring in terms of the standing or rank of the individual in each trial. That is, an individual is scored as greater than (numerical value 3), equal to (2), or less than the mean (1). This method gave a somewhat raised correlation, the odd versus even coefficient being 0.509 as compared with 0.379 for the crude scores.

The second study, reported by Davis and Tolman (7), was a continuation of this study of reliability of the maze scores. The only evidence as to the reliability of the maze in the first report was the correlation between the two halves of the same learning record. The purpose of this second investigation was to see if the correlation between one maze and another could be obtained with rats. The point most interesting in the results was that the correlation between the two mazes was higher than was to be expected from the correlations within the single mazes. The correlation with the thirteen rats was 0.658.

With the report of Tolman and Nyswander (25) in 1927 of their study on "The Reliability and Validity of Maze Measures

for Rats'' the statistical side of the question began to receive more attention. They discussed the various factors affecting the size of the correlations, i.e. reliability coefficients, such as varying experimental conditions, the pattern of the maze, the extent of the spread of the group in the ability measured, and the technique used in calculating the coefficients. Making use of the data from fourteen experiments in which seven distinctly different maze patterns had been used, they found that the reliability coefficients obtained by correlating errors on odd and even trials, and errors on one half of a segment of the learning curve with those on the second half varied considerably for the different mazes, odd versus even trials from 0.44 to 0.83 and first half versus second half segment from 0.03 to 0.82. Of the different kinds of maze patterns the multiple T type gave the highest coefficients. A circular maze of the Watson type and a Carr maze were found to give relatively low correlations as compared with the T mazes. Of this latter type a fourteen unit one gave higher results than either a six or eight unit one. That is, a large number of units in a maze is evidently the same as having a great many items in a test, it increases the reliability of the instrument. The authors concluded further that differences in experimental technique were relatively negligible compared with the pattern of the maze in determining the size of the reliability coefficient.

This study of Tolman and Nyswander is an important one in the history of the development of the problem we are considering for the reason that, as has already been mentioned, it called to the attention of the investigators in this field the importance of the statistical questions involved in the concept of reliability. Their discussion of the significance of the techniques used in calculating the ''reliability coefficients'' will be considered in the following section in which the different methods that have been used for this purpose in maze work will be discussed and an attempt will be made to reach a clearer understanding of their meaning and statistical validity.

Stone and Nyswander (26) in 1927 reported an investigation of the reliability of rat learning scores from a multiple T maze in which they employed four different methods of calculating re-

liability: (1) correlation of the sum of the errors on the odd trials with the sum of the errors on the even trials; (2) correlation of the sum of the errors for any segment of the trial series; (3) correlation of the sums of all errors for the first half of the maze with the sums of all the errors for the second half of the maze; (4) correlation of the sum of the errors on the odd numbered blinds with the sum of the errors on the even numbered blinds. The third and fourth of these methods were new with this study. The correlations obtained by all four methods were very high especially in comparison with those found in the earlier studies, which were about 0.30. The lowest they obtained was 0.46 and the highest 0.97 with the large majority of them being 0.90 or above. The highness of these coefficients would seem to corroborate the conclusions of both Hunter and Tolman concerning the greater reliability of the multiple T type of maze pattern. Still further evidence of this superiority of the T pattern is found in a study by Stone of the reliability of a modified Carr maze in which he used three of the methods described above for determining reliability. The correlations from this Carr maze were found to be consistently lower than from the multiple T pattern. If the reliability coefficients obtained from the multiple T maze by the four methods described above are a true indication of its reliability it will be seen that it compares quite favorably in this respect with most educational and intelligence tests and is reliable enough to be used in the study of individual differences. It is to the consideration of these various methods of determining the reliability of maze scores that we turn next.

5. THE METHODS OF DETERMINING THE RELIABILITY OF THE MAZE

A. Analysis and Criticism of Methods

Nine methods in all have been used by the various investigators in their attempts to determine the reliability of the maze. They are:

1. Correlation between learning score and relearning score on the same maze.
2. Correlation between a maze and another learning problem.

3. Correlation between a series of trials and another series of trials after considerable interval of time.
4. Correlation between scores on the various trials.
5. Correlation between the records on the different segments of the learning curve.
6. Correlation of the sum of the errors on the odd trials with the sum of the errors on the even trials.
7. Correlation of the sum of the errors on the odd numbered blinds with sum of the errors on the even numbered blinds.
8. Correlation of the sum of all the errors for the first half of the maze with the sum of all the errors for the second half of the maze.
9. Correlation between the results of two mazes.

The first two of these methods we have already considered to some extent in the previous section, the first in connection with Webb's study and the second in connection with Heron's study of the problem box. It was pointed out, in the case of each, that the scores correlated were not measures of the same thing. Thus in Method 1 we are correlating a measure of maze learning ability with a measure of relearning ability, or the ability to re-establish a previously acquired habit. The factor of retention enters in here to make the two functions different. Similarly in correlating maze scores with scores from some other learning problem, for example, the problem box, we do not obtain a measure of the reliability of either of these instruments. In the field of mental tests one would not consider the correlation between two intelligence tests a measure of the reliability of either one of them. Validity should not be confused with reliability. Method 3 is somewhat similar to Method 1, depending upon how much is learned in the first series of trials, and as such the same criticism applies to it as to Method 1.

In methods 3, 4 and 5 the assumption seems to be, as Tolman and Nyswander (28) have pointed out, that the "maze learning capacity" is a unitary function, of which any segment of the learning curve is a representative measure. Granting the truth of this assumption, it is nevertheless obvious that this function is operating in conjunction with certain other conditions and

that these conditions are quite likely to be inconstant from trial
to trial, although for large groups of trials they were probably
about the same on the average.   In the case of Method 4, then, in
which the scores on individual trials are correlated these fluctua-
tions are not given a chance to average themselves out and the
resulting correlations are very likely to be low.   Carr (3) has
argued in this connection that "a maze record for any trial is a
fairly accurate and reliable measure of that individual's perform-
ance for that maze and for the particular conditions that obtained
during that trial, but it cannot be used as a measure of that
individual's ability, i.e. as an index of what an individual will
do on another trial or what he will do on another maze."   Such
correlation coefficients are of no significance as measures of the
reliability of the score from a large number of trials.

In Method 5, in which the scores on a group of trials are cor-
related with those on another group later in the learning curve,
for example, first half of the trials versus the second half, the
question of the truth or falsity of the assumption that the maze
learning capacity is a unitary function becomes an important
matter in interpreting the coefficient obtained for any maze.
Tolman and Nyswander (28) have discussed this method at some
length and they have argued that when high correlations are
obtained with it the assumption is shown to be a true one and the
maze used is a reliable maze of the "piecemeal type," i.e. one in
which, to quote them, "the individual blinds are more or less
independent units, so that learning is a matter of successive
independent steps, and such that the successive heights of the
error curve mark the successive progress in the taking of those
steps" (28, p. 437).   Low correlations with this method mean
one of two things.   It may mean that we have an unreliable
"piecemeal type" of maze or that the maze is not of this type,
i.e. that the assumption does not hold for this particular maze.

We must now turn to the consideration of another factor that
enters into this method, and also to a greater or lesser extent into
methods 1, 3, 4 and 6, to further complicate the interpretation
of the correlation values obtained by them; it is that the variables
correlated are obtained from the same test.   That is, the two

sets of scores are from the same blinds. This involves a correlation between errors for there is a memory transference from one testing to another and whatever elements of error or chance occur in one series of trials are quite likely to occur in the other series. Consequently, when we correlate the scores of two series of trials made on the same maze, there is a factor operating that tends to make the coefficient obtained larger than the true reliability coefficient. This factor probably does not operate to any great degree in methods 1 and 3, especially if the performances correlated are separated by an interval of time long enough so that no conscious memory of the earlier performance could influence the latter. Even in such a case, however, there might still be an influence of one series on the other, resulting in a correlation between the errors of measurement.

It is apparent, then, that coefficients of reliability determined by methods one, three, and four are affected by two directly opposing sets of influence. One, the fact that the two measures correlated are not measures of the same thing tends to make the coefficient too small, while the other, the fact that there is a correlation between the errors of measurement, tends to make it too large. Just which of these two factors is the more prominent it is not always possible to say a priori. In the case of these three methods the correlation between the errors of measurement is probably relatively unimportant as compared with the opposing factor for, as we have already pointed out, the two measures correlated in each case are not at all comparable. Correlations obtained by these three methods, then, are in all probability too low as measures of reliability.

Method 5 is more ambiguous. If the maze is not of the "piecemeal type," then, the two values correlated are not measures of the same thing. This would tend to produce too low a coefficient. At the same time the fact that there is a correlation between the errors of measurement would tend to make it too large. Here again it is not possible to say, a priori, which set of factors has the greater influence. It is possible that they might balance each other and thus give a fairly accurate estimate of the reliability of the maze. In the case of a "piecemeal type" of

maze only the factor of the correlation of errors would be operative and the resulting coefficient would thus be higher than the true reliability coefficient.

The sixth method, the correlation between the performances on odd and even trials has been used more than any other method. Here again the learning record is divided into two parts. In the case of this method, however, it does not make any difference if the various segments of the learning curve are not measures of one and the same function, for the two sets of measures correlated are not from altogether different segments but each samples the same segments. The two measures so obtained, then, are fairly comparable in so far as being measures of the same thing and thus the factor that would tend to produce too low a reliability coefficient is not present to any extent. On the other hand the correlation between the errors of measurement is most high, for in this method, in which the scores on a series of trials (odd) are correlated with those on the immediately succeeding trials (even) there is certain to be a high degree of memory transference. We may conclude, then, that a reliability coefficient obtained by this method is most likely to be an overestimate. This method probably errs in this direction more than any other.

Methods 7 and 8, the correlation of the errors on the odd blinds with those on the even blinds, and the correlation of the errors on the blinds in the first half of the maze with those on the blinds in the second half respectively, are analogous to the standard procedure used in determining the reliability of mental and educational tests when only a single form of a test is available for use. That is, the score on one half the items of the test is correlated with that obtained on the other half the items. The correlation between these two sets of half scores is a measure of the reliability of either half the test. Then by means of the Brown-Spearman formula an estimate of the reliability of the whole test may be had. In so far as the various items of the two halves of the test are comparable, that is, in so far as they are similar in difficulty and type, and yet not so identical in word or form as to lead to a correlation between errors, a fairly accurate estimate of the reliability of the whole test can be obtained. It

is somewhat of a question, however, whether this same procedure with the maze, the blinds corresponding to the different test items, gives an accurate estimate of the reliability coefficient, due to the fact that the blinds of a maze are not independent items as are the different questions of an intelligence test. That is, there is such a great degree of interconnectedness between the blinds, especially between one blind and the next, that the measures correlated in these two methods are not independent of each other. This situation is different from that in methods five and six, in which the two sets of measures correlated are obtained by repeating the same test, i.e., the same blinds. We have not, then, a correlation between the errors of measurement in the sense of the previous methods, for the two sets of scores correlated in this case are not from the same test but from different ones, i.e., different blinds. This interconnectedness of the blinds however, like the correlation between errors of measurement, might probably tend to make the obtained reliability coefficient spuriously high.

In Method 7, in which the sum of the errors on the odd blinds is one measure and the sum of the errors on the even blinds the other, such a factor would play a prominent part for here the successive blinds are included in different halves of the test. This method, then, like Method 6 would tend to give a correlation coefficient higher than the real reliability coefficient.

In Method 8, in which the errors on the first half of the maze and those on the second half are correlated, the two measures are more likely independent of each other than in Method 7, at least it would seem reasonable to suppose so. This method, then, should give the more accurate measure of reliability. In fact it would appear to be the best measure of reliability that can be obtained from the results of only one maze.

There still remains to be discussed Method 9, the correlation of scores from two different mazes. The validity of this method depends upon whether the two measuring devices are comparable. Kelley (17, p. 203) in his discussion of the reliability coefficient gives three conditions that must be observed before two tests may be considered comparable. "(1) Sufficient fore-exercise should

be provided to establish an attitude or set, thus lessening the likelihood of the second test being different from the first, due to a new level of familiarity with the mechanical features, etc.; (2) the elements of the first test should be as similar in difficulty and type to those in the second, pair by pair, as possible; but, (3) should not be so identical in word and form as to commonly lead to a memory transfer or correlation between errors." The second and third of these conditions would require that the two mazes be of the same type (e.g. both high relief finger mazes of the multiple T type of pattern) and yet the arrangement of the various turns and blinds must not be so identical as to permit of memory transference. In the present experiment these two conditions have been more or less satisfied. The first of Kelley's three conditions would require that a few trials be given on a preliminary maze of the same type as the two test mazes so that the subjects would become familiar with the nature of the problem confronting them. The correlation between the two test mazes would not then be affected by the factor of unfamiliarity. Such a procedure would be analogous to the sample questions given at the beginning of intelligence tests. In the present experiment, unfortunately, no preliminary practice was given so this condition has not been met, and as a result, the obtained coefficient is lower than the true reliability coefficient.

It should be noted that a reliability coefficient determined by this latter method would tend to be lower than one determined by Method 8 even though in the case of this latter there were no interrelation between the learning of the two halves of the maze. The reason for this is that the two measures used in Method 8 are obtained in the same experimental period; the reliability coefficient is not affected by the factor of the variability of the function within the individual from one period to another. The coefficient obtained by Method 8 is affected only by chance factors (errors of measurement) due to the measuring instrument itself. The reliability coefficient obtained by correlating measures obtained at different times (Method 9), on the other hand, is affected, being lowered, by the fact that the ability or function is not a perfectly constant quantity. It is a measure of the errors

of measurement due to the instrument as well as those that are not due to it, such as, for example, changing physiological conditions, interest, etc.; or, to put it in another way, it is a measure of the reliability of the experimental technique, including both the conditions and the measuring instrument.

One further point concerning the methods of determining the reliability remains to be considered, and that is the question as to how much of the learning series is to be included in the maze scores that are correlated. Stone and Nyswander, for example, computed correlations for various segments of the learning series, trials 1–30, 1–20, 1–10, 11–30, 11–20 and 21–30. Most of the workers in this field have followed somewhat this same procedure, usually selecting as the length of the segment a point (trial) at which the best subjects reach perfection. They then divide the errors made up to this point into two parts according to the different methods. Such scores, it should be noticed, do not include the complete learning record of all those subjects who have not yet completed the learning of the maze. In computing the correlations in the present study a different procedure was followed. Instead of correlating the scores of a fixed segment of the learning curve (one which is identical for all subjects) the total maze score of each subject was used regardless of the differences in the number of trials required to learn for different subjects. The argument for adopting this method in preference to the other is that differences in individual maze learning ability involve differences in the number of trials required to complete the learning of the maze as well as differences in the number of errors made on the trials. To eliminate the former is to limit the differentiating ability of the instrument. It is not meant to be implied, however, that the method of using maze scores from a fixed number of trials is not a valid one. It is merely being pointed out that such scores do not differentiate the individuals within a group to as great an extent as do the complete learning scores and thus they do not give as high a reliability coefficient.

Tables V and VI clearly reveal the difference between these two scores in this respect. They give the reliability coefficients as determined by three different methods (6, 7, 8), the average of

the sigmas of the scores correlated, i.e. odd and even trials, etc., and the average of the coefficients of variation of these scores. The data are computed from the records of the group of men for Maze I.

A comparison of these two tables shows the variability of the group to be considerably greater for the complete maze score than that from ten trials. The relative variability is probably more accurately indicated by the coefficients of variation than by the sigmas. It will also be seen that the reliability coefficients are

<div align="center">TABLE V</div>
<div align="center">*Score on Ten Trials*</div>

METHOD	AVG. σ.	AVG. C.V.	REL. COEFF.
6. Odd Trials vs. Even*...................	6.8	33.5	.90 ± .03
7. Odd Blinds vs. Even....................	6.5	35.6	.61 ± .09
8. 1st Half Blinds vs. 2nd.................	6.7	37.2	.45 ± .11

* Include errors due to retracing.

<div align="center">TABLE VI</div>
<div align="center">*Complete Maze Score*</div>

METHOD	AVG. σ.	AVG. C.V.	REL. COEFF.
6. Odd Trials vs. Even*...................	15.8	56.0	.95 ± .01
7. Odd Blinds vs. Even....................	14.7	56.6	.88 ± .03
8. 1st Half Blinds vs. 2nd.................	14.9	57.3	.70 ± .07

consistently higher in the case of the complete maze scores for each method.

This concludes the discussion of the various methods that have been used in determining the reliability of the maze. An attempt has been made to analyze each one and to point out just what significance the "reliability coefficient" obtained by each has. As the status of the methods vary to some extent under different experimental conditions it is not possible to state definite conclusions that will hold for all maze experiments. Given however two comparable mazes in which also the two sets of halves of each, odd and even, 1st half and 2nd half blinds, are comparable

and given as far as possible uniform experimental conditions the following conclusions would seem to be warranted:

I. Only four (6, 7, 8, 9) of the nine methods give unequivocal measures of the reliability of the maze apparatus as a measure of "the ability to learn a maze" for in these methods the two scores that are correlated are both more or less alternate measures of this ability.

2. In methods 1, 2, 3, and 4 the scores correlated are not both measures of the same ability. They tend as a result to underestimate reliability.

3. The coefficient obtained by Method 5 is difficult to interpret. If the maze is of a type such that the two segments of the learning curve correlated are measures of the same function then the obtained coefficient would be an overestimation because of the correlation between errors of measurement. If the two segments measure different functions the overestimation would tend to be reduced if not eliminated altogether.

4. Methods 6, 7, and 8 all tend to overestimate the reliability of the maze, Method 6 to a considerable extent because of the correlation between errors of measurement, and Methods 7 and 8 to a lesser extent on the assumption that the interconnectedness between the blinds tends to increase the coefficients but to a lesser degree than the correlation between errors of measurement. On the further assumption that this factor of interconnectedness is less in Method 8 than in Method 7 the former overestimates to a lesser degree than the latter.

5. Method 9, influenced as it is by the variability of the function within the individual, and uncontrollable variations in the experimental technique as well as those due to the structure of the maze itself represents the lower limit of the reliability coefficient. As a measure of reliability it takes into account all these factors.

B. Consideration of a Recent Point of View

In the interval between the first writing of the preceding analysis of the methods of determining the reliability of the maze as a part of a M.A. Thesis (May, 1930) and the present prepara-

tion for publication a very important article by R. C. Tryon (29) concerned with this problem of the reliability of maze scores has appeared in the Journal of Comparative Psychology. This study of Tryon's, formulating as it does eight experimental-statistical principles of measurement derived from the theory of human mental measurement which should be used in the construction of reliable scales of maze ability in rats, represents a considerable contribution to the subject. Particularly relevant to the present study is his discussion in connection with the seventh principle, that the reliability is increased the more comparable are the two sets of measurements correlated, other things being equal, for in it he develops a position that is in disagreement with a part, at least, of the conclusions reached in the previous section.

According to Tryon the only exact method of determining the reliability of maze scores is what he terms the split-test method, which consists either in correlating the scores on odd and even trials (Method 6) or the scores on odd and even blinds (Method 7). Later in the paper he points out that the latter method gives values lower than the former, probably, he explains, because the odd blinds are not strictly comparable with the even. All other methods, he claims, give values that are too low as measures of reliability. It is to be noted that so far as the relative size of the coefficients obtained by these methods are concerned, his conclusions are in harmony with those of the present writer. It is on the question of which method is the most valid that difference arises. Tryon claims Method 6 to be the most accurate (Method 7 would be equally so providing the odd and even blinds are always comparable) and that the other methods are underestimates, while the contention of the present writer is that Method 6 gives a decided overestimation of reliability and that Methods 7 and 8 also err in this direction but to a lesser degree.

In order to make clear Tryon's position in this matter it is reproduced below. He begins by pointing out that when the conditions of comparability are satisfied, the standard deviations of each set of measurements are equal. Continuing he writes, "For such experimentally determined material as scores on successive trials on a maze, Spearman devised the method of collect-

ing odd elements in one set (the elements may be trials or blinds), correlating these two independent sets, and then applying the correction which eventually became known as "Brown's formula." The corrected correlation is the reliability coefficient of total scores on all trials or blinds. This method, called the "split-test" method, is the only exact method of determining the reliability coefficient of maze scores. All other methods, such as that of correlating the first half of the trials or blinds against the last half, or correlating one maze against another, do not satisfy the definition of the reliability coefficient, for the two sets of measures used in the other methods are rarely comparable according to the criterion of comparability. The standard deviations of such sets are, of course rarely equal. As a consequence of lack of comparability, correlations between such sets of measures give values lower than the true reliability coefficients" (29, p. 154).

In applying the method devised by Spearman to maze scores, however, Tryon seems to have failed to notice that the two sets of trial scores (odd and even) are not independent of each other as was shown in the previous section. Spearman (24), in his exposition of this method, gave as an example of it a test of verbal memory consisting of memorizing twenty series of words, in which case the scores made on the odd and even groups were correlated. These two scores, it should be noticed, however, are independent of each other because they are made on different series of words. In the maze, on the other hand, the odd trial scores are made on the same test, that is on the same series of blinds, as the even trial scores, and the coefficient obtained is accordingly between repeated tests. As Kelley points out (17) this is a vitiating of Spearman's concept of reliability. In correlating blinds, however, the two sets of scores are not from the same test, but from two different sets of blinds, odd and even or first and second half. The question of whether even these scores are altogether independent of each other was discussed, it will be remembered, in the previous section. To conclude, we note again that the odd versus even trials correlation coefficient violates the third of the three conditions given by Kelley that must be observed before the two sets of measurements can be

considered strictly comparable, and, as a result, tends to give a
value higher than the true reliability coefficient. The practical
criterion of comparability, the equality of the standard deviations
of the two sets of measurements, it should further be noted, is of
little value in connection with this condition as non-compliance
with it, such as repeating the same test, does not necessarily lead
to differences in the standard deviations.

Tryon's statements concerning the methods of correlating the
first half of the blinds against the last half (Method 8) and cor-
relating one maze with another (Method 9) also requires some

TABLE VII

Standard Deviations. Group I

MAZE SCORES	MAZE I		MAZE II	
	σ.	P.E. σ.	σ.	P.E. σ.
1st Half Trials............................	19.7	1.3	15.7	1.0
2nd Half Trials...........................	10.5	0.7	7.5	0.5
Odd Trials...............................	15.5	1.0	11.6	0.8
Even Trials..............................	14.6	1.0	11.2	0.7
Odd Blinds..............................	15.3	1.0	13.9	0.9
Even Blinds.............................	12.2	0.8	9.2	0.6
1st Half Blinds..........................	16.3	1.1	11.8	0.8
2nd Half Blinds.........................	12.9	0.9	12.1	0.8

discussion. Although it is true that the two sets of measures
correlated in these two methods are not always comparable, it
does not seem that it would be too difficult a task to make them
so. Method 9 would require that sufficient fore-exercise be
given to eliminate the factor of strangeness and that a careful
equating of the difficulty of the blinds of each test be made.
The problem is much the same with respect to the measures
obtained from the first and second half blinds of a single maze.
Here the two halves would have to be made comparable in diffi-
culty. As may be seen from Table VII, which gives the standard
deviations of the different sets of scores in the present experiment,

it is doubtful whether the odd and even blind scores are comparable in either Maze I or Maze II. The difference between the standard deviations in Maze II, at least is great enough to insure that the difference is a true one. The first and second half blinds of Maze II on the other hand are comparable as the standard deviations of their scores are equal within their sampling errors. In Maze I there is quite a difference in the standard deviations for these same two scores, but it is questionable whether it is a true one.[2]

6. EXPERIMENTAL EVIDENCE BEARING ON FIVE METHODS OF DETERMINING RELIABILITY OF MAZE SCORES

A. Discussion of the Results of Present Experiment

It is the purpose of this section to ascertain the extent to which the conclusions concerning the methods of determining the reliability of the maze reached in the previous section fit the experimentally determined facts. Only methods 5, 6, 7, 8, and 9 will be considered in this part of the study. The remaining methods

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[2] Since the present paper was accepted for publication another study dealing with this problem of the reliability of maze measures has been reported. (Leeper, R. The Reliability and Validity of Maze Experiments with White Rats. Genetic Psychology Monograph, 1932, XI, 141–237.) Leeper very clearly points out that reliability coefficients from maze experiments are likely to be biassed in one direction or other. He has emphasized the fact that, under faulty experimental conditions, certain irrelevant and systematic factors such as differential motivation for example, will result in a spuriously high measure of the reliability of the maze as an instrument for measuring learning ability. Such experimentally controllable factors as differential motivation age, etc., should be carefully distinguished, however, from such things as position habits, which Leeper seems to include in the same class. These latter are not irrelevant to the maze. In such methods as correlating odd and even trials, however, they have the same effect of producing a spuriously high reliability coefficient for we have here what we have been referring to as a correlation between chance errors, due to the fact that we are correlating scores from the same test.

Leeper seems to have been somewhat uncritical in his conclusions concerning methods 7 and 8, odd blinds versus even blinds and 1st half blinds versus 2nd half blinds. These measures of reliability, he claims, are probably too seriously effected by systematic factors and are thus more lacking in independence than odd and even trial scores (Method 6), a conclusion that is contrary to the one reached as the result of our own analysis, and, most convincing of all, contrary to the experimental facts both of the present investigation of human maze learning and of all animal investigations on which there is available data.

fail to so great an extent to comply with the conditions underlying the definition of the reliability coefficient that they are not considered further.

These conclusions, it will be remembered, were contingent upon the observance of certain ideal conditions. When considering any particular experiment then, account must be taken of any variation from these conditions. In the present experiment there are one or two such factors to be considered. Mention has already been made of the fact that no practice was given previous to the learning of Maze I. This situation affects primarily the validity of Method 9, making the coefficient obtained by it too low as a measure of reliability. The probability that the odd and even blind scores, in the case of Maze II at least, are not comparable must also be considered. Such a factor would tend to offset the assumed influence of the interconnectedness of the blinds, reducing if not eliminating the overestimation resulting from it.

If then, the analyses of the various methods and experimental conditions that have been made are correct, and providing the assumptions made hold, the following are the results that should be expected in the present experiment:—Method 6 should give the highest coefficient and Method 9 the lowest. The value obtained by Method 8 should be somewhat lower than that obtained by Method 7, although in the case of Maze II it might be expected that this difference would be reduced or eliminated altogether, depending upon the relative strength of the factor of interconnectedness and the factor of non-comparability of the odd and even blinds. The order of these four methods in size from the highest to the lowest, then, should be 6, 7, 8, 9. The relation that should obtain between Method 5 coefficient and the others is more difficult to state. As in Method 6 the correlation between the errors of measurement plays a prominent part, thus tending to make the obtained reliability coefficient spuriously high. The scores from the two segments of the learning curve, however, do not appear to be measures of the same thing, as may be seen from Table VII, which shows their standard deviations to differ considerably. This latter factor would tend to offset the influence of the correlation between the errors of measurement.

Method 5, then, should be expected to give a lower coefficient than Method 6. How it should compare with Methods 7 and 8 it is not possible to state. Whether it would be higher or lower depends upon the relative strength of the different factors operating in them.

The extent to which these theoretically determined relations are corroborated by the experimental results may be seen from an examination of Tables VIII to XI, which give the coefficients obtained by the various methods in the present experiments. In Table VIII the correlations between the two mazes (Method 9) for the college group (I) are given. The first row values in this table are product-moment r's, while the coefficients in the second row were calculated by the rank method. It is to be noted that the latter values are somewhat higher, except in the case of time,

TABLE VIII
Reliability Coefficients (Method 9)

MAZE I VS. MAZE II	S. ERRORS	D. ERRORS	TRIALS	TIME
r (product-moment)................	.60 ± .06	.63 ± .06	.54 ± .06	.73 ± .04
r (rank method)....................	.70 ± .05	.71 ± .05	.62 ± .06	.72 ± .05

than the corresponding product-moment values. This might possibly be due to the fact that the rank method, being the cruder, is affected to a lesser extent by the unfamiliarity factor. The rank method values would, then, give the more accurate indication of the reliability of the scores.

Tables IX and X show the coefficients obtained for the college group by methods 5, 6, 7, and 8 in Maze I and Maze II. Table XI gives the same data for the Y. M. C. A. group in Maze I. It will be seen that the relative values of the different methods are remarkably consistent from table to table, Method 6 giving, in each case, the highest coefficient and Method 8 the lowest. In the college group Method 5 gave the next highest coefficient. In the Y. M. C. A. group, however, Method 7 gave the second highest coefficient, Method 5 being third. The coefficient for single errors obtained by Method 9, it will be noticed, is lower than any of the values obtained by these four methods.

That these results conform very closely to the theoretical expectations is readily apparent. Methods 6, 7, 8 and 9 are in the exact order of size demanded by our analysis, and Method 5 is in each case lower than Method 6. The evidence of this study, then, certainly tends to confirm the accuracy of our theoretical

TABLE IX

Maze I. Group I. (Single Errors)

METHOD	OBTAINED r	S-B. ESTIMATE (r_{II})
5. 1st Half Trials vs. 2nd........................	.88	.936 ± .01
6. Odd Trials vs. Even...........................	.95	.975 ± .01
7. Odd Blinds vs. Even..........................	.85	.918 ± .02
8. Ist Half Blinds vs. 2nd........................	.71	.830 ± .04

TABLE X

Maze II. Group I. (Single Errors)

METHOD	OBTAINED r	S-B. ESTIMATE (r_{II})
5. 1st Half Trials vs. 2nd........................	.80	.888 ± .02
6. Odd Trials vs. Even...........................	.92	.958 ± .01
7. Odd Blinds vs. Even..........................	.77	.875 ± .02
8. 1st Half Blinds vs. 2nd.......................	.73	.844 ± .04

TABLE XI

Maze I. Group II. (Single Errors)

METHOD	OBTAINED r	S-B. ESTIMATE (r_{II})
5. 1st Half Trials vs. 2nd........................	.80	.888 ± .03
6. Odd Trials vs. Even...........................	.95	.975 ± .01
7. Odd Blinds vs. Even..........................	.91	.953 ± .01
8. 1st Half Blinds vs. 2nd.......................	.75	.857 ± .04

analysis of the methods of measuring maze reliability. Such perfect conformity in all three cases is hardly a chance result.

It should be noticed, however, that the experimental evidence in itself does not throw much light on certain of the assumptions that were made. Particularly is this so with respect to the assumption that the factor of the interconnectedness of the blinds is operating in Method 8 to make it an overestimate. That such

a factor is operating in Method 7 would seem to have been definitely demonstrated. Even in the case of Maze II, in which there was also an opposing factor operating, as has been pointed out, Method 7 gave a higher coefficient than Method 8. The greater degree of interconnectedness between the odd and the even blinds than between the first and the second half blinds seems to be a fairly plausible explanation of this fact. We have no direct evidence, however, as to the extent to which this factor is operating, if at all, in Method 8, and, as a result, we do not know whether the coefficient given by it is an overestimate or not. As far as the college group is concerned, then, we can be sure only that the true reliability coefficient of the present maze scores lies between 0.60, the coefficient obtained by Method 9 for single errors, and 0.84 the coefficient obtained by Method 8. The former we know to be an underestimate; the direction of the error of the latter, if any, is doubtful.[3]

B. Evidence from Animal Studies

Only a few of the animal maze studies have data bearing upon the methods under consideration. Stone and Nyswander (26) present tables showing the values they obtained for methods 5, 6, 7 and 8. Table XII shows the coefficients they obtained for five different age groups in trials 1–30. The values are Spearman-Brown estimates of the coefficients of combined scores. In the last column an average of the five values obtained by each method is given. These data, it will be seen, are in fairly close agreement with the results of the present experiment. Thus Method 6 gave coefficients higher than any of the other methods. Methods 7 and 8, however, gave about the same values, while Method 5 was on the average the lowest of the four. This latter fact suggests that the two segments of the learning curve in the case of this maze are not at all comparable. As the standard deviations of the scores from which these correlations were

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[3] No account has been taken here either of the accuracy of the estimate of the Spearman-Brown formula or of its applicability to the present data. Concerning the former there is some experimental evidence showing that the formula does not hold, tending to give an overestimation.

computed are not given there is no means of judging as to their comparability.

A possible explanation of the relative highness of Method 8 coefficients in this investigation is that the true reliability coefficient of this rat maze is really fairly high. In such a case the value given by Method 8 would tend to approach those of Method 6 and Method 7. That is, if we have a practically perfectly reliable device these three methods would all tend to give the same result. It is when the reliability of the measuring instrument is low that methods 6 and 7 are most in error. The high reliability of these rat maze scores is clearly evidenced by the fact that the coefficients obtained were determined from the scores of a

TABLE XII
Reliability Coefficients. Stone and Nyswander

METHOD NO.	AGE GROUP (IN MONTHS)					AVG.
	5	6	8	9	12(a)	
5	.95	.85	.77	.94	.69	.84
6	.99	.97	.96	.95	.95	.96
7	.95	.95	.87	.93	.85	.91
8	.90	.96	.93	.82	.86	.89

fixed number of trials. Complete learning scores would have given higher coefficients.

Stone (25), in a study of the reliability of a modified Carr maze, gives data that permits a comparison of methods 4, 6 and 8. Table XIII gives the values he obtained by these methods for three different age groups. The 50 day and 120 day groups include both male and females while the 2 year group contains only females. Method 6 in every case, it will be noticed, gave results higher than Method 8. The values in this table as in the previous are Spearman-Brown estimates.

Further data from the field of animal maze work are provided by Ruch's study (21) of motivating conditions in maze learning. Table XIV shows the values he obtained for two different groups of rats on the same maze. One group's motivation was hunger while the other's was desire to escape from water. The maze

used was a multiple T pattern. The values (which are obtained r's) for the Escape-From-Water group, it is to be noted, are entirely in harmony with the theoretical expectations. The hunger group, however, presents an exception in that Method 7 gave the highest value and not Method 6. Also the difference between Method 6 and Method 8 is small for such low reliability.

Tryon (29), as has already been stated, found that the odd versus even blind coefficient was lower than the odd versus even

TABLE XIII
Reliability Coefficients, Modified Carr Maze

METHOD NO.	AGE GROUP		
	50 Days (N 78)	120 Days (N 95)	2 Years (N 28)
5	.36 ± .07	.21 ± .09	.69 ± .05
6	.75 ± .02	.56 ± .04	.75 ± .04
8	.67 ± .03	.32 ± .07	.58 ± .07

TABLE XIV
Reliability Coefficients, Ruch

METHOD NO.	GROUP	
	Hunger	Escape-from-water
5	.460 ± .12	.571 ± .10
6	.682 ± .08	.896 ± .03
7	.827 ± .05	.645 ± .09
8	.590 ± .10	.513 ± .11

trial coefficient. He has also reported a coefficient between two mazes (Method 9) of $0.77 \pm .02$. The coefficients obtained by Method 6 for these two mazes were 0.987 and 0.968 (both corrected by the Spearman-Brown formula).

7. THE RELIABILITY OF THE PRESENT MAZE

It was concluded, it will be remembered, that the true reliability coefficient of the maze scores (single errors) for the present experimental group was between 0.60 and 0.84. As a basis for further discussion we may accept the rank-method r of 0.70, obtained by

Method 9, as a fairly accurate indication of this reliability. It represents, at least, the lower limit of the reliability coefficient. What, then, is the significance of this figure? Does it indicate a high enough reliability to warrant our ever regarding this instrument as a reliable device for the measurement of individual differences?

The answer to this latter question depends upon the extent of the variability of the group for which the test is intended. For, if we may assume that the instrument is equally accurate for different ranges of ability, that is, that the standard error of measurement ($\sigma \sqrt{1 - r}$), which is a measure of the amount of variable error in an obtained score, is the same for groups of different ranges, then, it is apparent that the effectiveness of the

TABLE XV
Showing Variability of Groups

GROUP	OTIS	MAZE I STANDARD DEVIATIONS			
	S.D.	S. Errors	D. Errors	Trials	Time
I. (Main).........................	6.4	29.4	37.7	10.6	377.5
II. (Boys.........................	11.0	48.4	78.3	16.1	614.7

instrument in differentiating individuals within a particular group, being dependent upon whether or not the differences between the individuals in the ability measured are relatively large as compared with the error of measurement, will vary with the magnitude of these differences. That is, the same measuring instrument will be more accurate at differentiating individuals in a group with a relatively large spread (i.e. a large average individual difference) than in a group with a relatively small dispersion (i.e. a small average individual difference). The reliability coefficient is an indication of this effectiveness for the particular group from which it was obtained.

The reliability coefficient alone, however, is not a good basis on which to judge as to the reliability of any measuring instrument for it varies in size with the range in ability of the group, being highest in groups with the greatest range. Thus while the

coefficient of 0.70 obtained for the present maze is not sufficiently high to insure that it accurately differentiates individuals, a coefficient of 0.90 is usually required for such purposes, it is nevertheless, a fairly significant value when one considers that the group for which it was obtained represents a very highly selected and narrow range of ability.

The standard deviation of the intelligence test for this group was 6.4 as compared with 11.0 for the Y. M. C. A. group and 13.8 for a group of pupils in grades 7 to 12. That the degree of curtailment of its range is about the same for maze learning ability as for intelligence may be seen from a comparison of the standard deviations of the groups of different ranges on the two tests. Table XV gives the standard deviations of the main group and the boys' group for Otis scores and for Maze I scores. An in-

TABLE XVI
Relation of Variability of Groups

GROUP	OTIS	S. ERRORS	D. ERRORS	TRIALS	TIME
I. (Main).........................	1.	1.	1.	1.	1.
II. (Boys).......................	1.72	1.65	2.08	1.52	1.63

spection of the table reveals the fact that the relation between the size of the standard deviations of the two groups in the maze is much the same as in the Otis test. Table XVI shows this correspondence more clearly. It gives the ratios of the standard deviations of the boys' group to those of the main group. Thus the boys' group is 1.7 times as variable as the main group in the Otis test and 1.65 times as variable as the main group in the maze (single errors).

It is clearly evident, then, that this college group represents a very narrow range of maze-learning ability. To obtain a reliability coefficient as high as 0.70 in such a greatly curtailed range of ability is an indication that the test is a fairly reliable one. Using Kelley's formula

$$\frac{\sigma}{\Sigma} = \frac{\sqrt{1-R}}{\sqrt{1-r}}$$

for estimating the reliability coefficient of a test over one range of talent, when its reliability over another is known, we obtain a reliability coefficient of 0.857 (single errors) for a group with a spread as great as that of the elementary psychology class and a coefficient of 0.925 for a group such as the 7–12th grade pupils (assuming the standard deviation of this group is twice that of the present experimental group for the maze as it is for the Otis test). These values incidentally are the same as the reliability coefficients of the Otis test for these two groups, which were 0.855 and 0.925.

This brings us to the question of the accuracy of the estimates given by Kelley's formula. The formula is based upon the assumption that the standard errors of measurement (i.e. the standard deviations of the divergences of the obtained scores from the true scores) are equal for different ranges of talent. This assumption, as Kelley himself has pointed out, is not a mathematical necessity but an experimental matter. Its validity for any particular test must first be verified by experimental investigation.

What evidence is there that the standard error of measurement of the present maze device is the same for different ranges of talent? A comparison of the standard errors of measurement of the college group and the boys' group determined by Method 9 reliability coefficients is not possible as the boys' group learned only Maze I. Using Method 8 reliability coefficients (Spearman-Brown estimates) we find that the standard error of measurement of Maze I scores (single errors) is 18.34 ±1.86* for the boys' group and 12.13 ±.88* for the college group. An analysis of the difference between these two values reveals the fact that the chances are 98 in 100 that the true difference is greater than zero.

It should be remembered in connection with this problem that the error of measurement is a function of faulty experimental technique as well as the fallibility of the measuring instrument. Any difference, then, between the errors of measurement found in

* P.E. $\sigma_m = .6745 \dfrac{\sigma_m \sqrt{3 - r^2}}{\sqrt{4n}}$ (18).

two groups differing in range of talent might be due to a difference in the experimental conditions. While the experimental technique was similar for the college and boys' groups, such factors as distraction, fatigue and interest were more difficult to control in the latter group. These factors account to some extent, no doubt, for the increased error of measurement in this group. It is also possible that the maze device is not as reliable at lower levels of ability as it is at higher levels.

Some evidence as to the nature of the standard errors of measurement of two groups which are of the same level of ability, but which differ slightly in range is shown by a comparison of the men and women sub-groups. The men's group has a standard deviation 1.15 times that of the women's group for the Otis test

TABLE XVII
Standard Errors of Measurement

$$\sigma_M = \frac{\sigma_1 + \sigma_{II}}{2} \sqrt{1 - r_{1II}}$$

GROUP	S. ERRORS	D. ERRORS	TRIALS	TIME
F. (Women)................	16.0 ±1.79	20.2 ±2.25	7.4 ±.84	166.0 ±18.0
M. (Men).................	16.3 ±1.76	19.8 ±2.11	6.1 ±.73	178.0 ±18.5

and 1.25 times that of the women's group for the maze test. (The latter relation is between the averages of the standard deviations of Maze I and Maze II single errors). Table XVII gives the standard errors of measurement and their probable errors of the various maze scores for these two groups. An inspection of this table shows that they are just about the same for the single and double error scores, while trials show a smaller error and time a larger error in the relatively wider range of talent (men's group). These differences are of no significance, however, because of the large sampling error present.

Table XVIII shows the actual results obtained with Kelley's formula. It gives in the columns under Group M and Group F the obtained product moment r's and average standard deviations (i.e. of Maze I and II) of these two groups. The last column gives the estimated reliability coefficients for a range the size of

the men's group. That is, these coefficients were estimated from the values obtained from the women's group by Kelley's formula. A comparison of these estimates with the actually obtained r's shows them to be extremely accurate except in the case of the trials, which is the most unreliable of the different types of maze scores. The fact that one of the women took the abnormal number of 57 trials to learn the first maze has considerably affected the results for this score. Table XVIII shows very clearly how the reliability coefficient varies with the extent of the range of the group from which it was obtained.

The question as to whether the error of measurement of the present maze device would be the same for still greater ranges of

TABLE XVIII

MAZE SCORE	GROUP M.		GROUP F.		$R = 1 - \dfrac{\sigma^2 (1 - r)}{\Sigma^2}$
	Obt'd r	Ave. Σ.	Obt'd r	Avg. σ.	
S. Errors	.67	28.4	.49	22.6	.67 ± .07*
D. Errors	.70	36.2	.49	28.4	.69 ± .07
Trials	.66	10.5	.37	9.4	.50 ± .14
Time	.79	389.6	.62	269.6	.81 ± .04

$$* \text{ P.E. } R = .6745 \, (1 - R) \left[\frac{2}{N} + \frac{3 - r^2}{n} \right]^{\frac{1}{2}} \text{ (5).}$$

ability (which would necessarily include lower levels of ability) cannot be definitely answered here. The fact that the standard error of measurement of the boys' group was greater than that of the college group for Maze I scores suggests that there is a minimum level of ability below which the present maze is less reliable. We may tentatively conclude, however, that the present maze will be equally reliable (i.e. have the same error of measurement) for different size ranges, providing comparable experimental conditions can be and are maintained for each range.

As to the significance of the reliability of the present maze we must conclude, in view of the fact that the coefficient of 0.70 represents at most the lower limit of the reliability coefficient for a group with a very narrow range of ability, that it compares

favorably in this respect with most intelligence tests. That is, the present type of maze is a fairly reliable instrument in the sense that chance does not play a predominant part in determining the score obtained on it. As the accuracy of any score is also a function of the uncontrolled factors in the experimental technique, the chief problem in the development of a still more accurate maze measure is this matter of the standardization of technique. The experimental technique should be carefully standardized and rigidly adhered to just as is the standardized procedure of any educational or intelligence test.

8. SUMMARY AND CONCLUSIONS

A. General Summary

The present study is concerned with the maze as an instrument in psychological research. The problem investigated is that of reliability, which includes a study of the various methods of determining the reliability of maze measures and a study of the reliability of the high relief finger maze with the multiple T type of pattern. The various methods of determining the reliability of maze scores are analyzed and the significance of their results indicated. Evidence supporting the accuracy of this analysis is given from the results of the present experiment and from animal maze studies.

B. Conclusions

1. Of the various methods that have been used to determine the reliability of the maze, Method 8 (correlation 1st half blinds versus 2nd half blinds) and Method 9 (correlation of the results of two mazes) are the most valid. The latter is the best measure of the reliability of any obtained score, while the former might better be interpreted as an indication of the upper limit of reliability, that is, it gives the reliability coefficient we might expect to obtain if all the variable factors other than the maze itself (i.e. the experimental conditions, subject's physiological state, attitude, etc.) were constant.

2. Method 6 (correlation odd trials versus even trials) tends decidedly to overestimate the reliability of the maze due to the

fact that there is a correlation between the errors of measurement in the two series of trials.

3. Method 7 (correlation odd blinds versus even blinds) also tends to overestimate the reliability of the maze, due to the fact that there is a high degree of interrelationship between the two halves of the maze correlated.

4. Method 5 (correlation 1st half trials versus 2nd half trials) is ambiguous. It tends to give too high a coefficient if the two halves of the learning curve measure the same function and too low if they do not.

5. The reliability coefficient of the present maze, single errors, is between 0.60 and 0.84, which, considering the narrow range of ability of the group measured is indicative of a fairly high degree of reliability.

6. The trial score is the most unreliable of the various maze measures, while time is the most reliable. Single error and double error scores are about the same, being slightly less reliable than the time score.
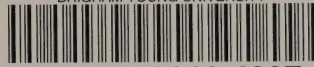
7. The standardization of the experimental technique is the most important problem in the development of a more accurate maze measure.

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